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NOTICES

OF THE

# PROCEEDINGS

AT THE

MEETINGS OF THE MEMBERS

OF THE

# Royal Institution of Great Britain,

WITH

ABSTRACTS OF THE DISCOURSES

DELIVERED AT

THE EVENING MEETINGS.

VOL. VII.
1873—1875.

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# Royal Institution of Great Britain.

## WEEKLY EVENING MEETING,

Friday, January 17, 1873.

Sir Henry Holland, Bart. M.D. F.R.S. President, in the Chair.

William Spottiswoode, Esq. LL.D. M.A. Treasurer R.S. and R.I.

### *On the Old and New Laboratories at the Royal Institution.*

A time when, through temporary absence from one chair, and through a change of occupancy of the other, we are deprived of the presence of our two Professors, seems to offer an opportunity for reviewing the past history, the scientific results, and the future prospects of our laboratories. A time when, through circumstances which cause us much regret, we are deprived, at our evening meetings at least, of the presence of our Secretary, offers perhaps the only occasion when the task of such a review could fall to other hands than his. The fact that it has fallen to mine is attributable to the office in which your votes have placed me, rather than to any individual qualifications of my own; and it would have been impossible for me to undertake the task, had he not placed at my disposal his wide-spread information upon many branches of science, as well as his intimate knowledge of the history of the Institution, to the well-being of which his care and devotion have so largely contributed.

The first dawn of our history is to be sought among those stormy years with which the last century drew towards its close, and out of which many new thoughts and aspirations of men took their birth.

Its character, in accordance with the views of its early promoter, Count Rumford, was at first far more industrial than it eventually became. Its two great objects were "the general diffusion of the knowledge of all new and useful improvements, and teaching the application of scientific discoveries to the improvement of arts and manufactures, and to the increase of domestic comfort and convenience." The Institution was to contain models, or actual specimens of fire-places and kitchen utensils for cottages, farm-houses, and large dwellings; a complete laundry for a gentleman's family; grates and chimney-pieces; brewers' boilers; distillers' coppers; ventilators; lime-kilns; steam-boilers; spinning wheels; agricultural implements; bridges, &c., &c.; and at one time some eighteen or twenty young mechanics were actually boarded and lodged in the house. The records of our early proceedings give an instance, illustrating the views of the founders. In January, 1800, when the designs for the theatre, model-room, and workshops were formed, the architect proposed that the laboratory should occupy the position which it ultimately held. But, with a view to giving more room to the workshops, the proposal was set aside in the very next month, and the space in the basement under the theatre assigned to the purpose. Happily, however, before the building had reached the first floor, this position was found unsuitable; and further consideration devised the laboratory, which we have all known so well as that of Davy, of Faraday, and of Tyndall. A staircase leading to it from the front hall, although long since closed, was removed only in 1866, to make room for Tyndall's smoke chamber.

From Count Rumford's final departure from England in 1802 we may date the decline of the industrial element, some echo of which still rings in our motto, "Illustrans commoda vitæ;" and early in the following year a definite proposal to give up that part of the original plan was made.

From a report to the managers in 1803, it appears that, although chemistry had always been a primary object of the Institution, yet from motives of economy nothing more had been done in the way of either laboratory or apparatus than was necessary for the immediate purpose of the lectures. It was consequently proposed that the workshop should be added to the laboratory and fitted with seats for 120 persons, and the forge adapted to chemical purposes. The report ends as follows:—"This laboratory will be equal, or indeed superior, to any in this country, and probably to any on the Continent."

The chemical laboratory was altered in accordance with that report, and remained unchanged until 1863, when, on the appointment of Dr. Frankland to the Professorship of Chemistry, the lecture seats were removed so as to adapt the room more properly to purposes of scientific research.

It is interesting to contrast the verdict of 1873 with that of 1803. "Originally built," to quote Dr. Bence Jones's own words, "as a workshop for blacksmiths, fitted with a forge, and furnished with bellows which only last summer left the Institution, our chemical laboratory was probably the very worst in London."

The physical laboratory remained unchanged; and although Professor Tyndall for himself desired nothing more than to continue his researches in a place which his imagination filled with the recollections of his predecessors, he still acquiesced in the proposal for rebuilding, for the sake of his successors, and in the interest of the sister science of his colleague.

Thus much about the material fabric of our laboratories. Next as to the scientific work of which they have been the birthplace.

Of the great names connected with this building foremost in order of time, and very high in scientific rank, stands that of Dr. Thomas Young. His 'Theory of Light and Colours' will always stamp him as one "whose genius has anticipated the progress of science," and whose reputation has risen as men have better understood his worth. His first paper on the subject was presented to the Royal Society in

November, 1801; but the earliest printed account of his views is to be found in his 'Syllabus of Lectures at the Royal Institution,' dated January 19, 1802.

With the criticisms of his theory published in the 'Edinburgh Review,' with the circumstances which led to his withdrawal from the Institution, with his researches in Egyptian hieroglyphics, we are not here concerned. But it is not too much to say of him, that without the Wave Theory of Light (of which he was one of the prime and main founders) to serve as a guiding-thread through the labyrinth of phenomena, the long series of discoveries which have in this place culminated in those of Tyndall in Radiation and Absorption, would have been impossible.

It is often remarked that little rills, which have threaded their way from distant mountains, ultimately discharge themselves as mighty streams into the sea. Yet between these two stages they flow quietly, but not therefore less usefully, past smiling meadows and the haunts of men. And here is a little scientific pastoral—if it may be so called—flowing out of the highest conceptions of the theory of undulations, and furnishing—to use his own words—a simple instrument "for measuring the diameters of the fibres of different kinds of wool."* [The lecturer then described and exhibited on the screen the principle of Dr. Young's eriometer.]

Our next name is that of Davy, an account of whose discoveries would require a volume, and a bare recital of them would be long. I quote the following notes from the pen of our Secretary, and wish that he had been here to give life to the dry bones.

In 1806, when twenty-eight years of age, Davy did the work which formed his first Bakerian Lecture, 'On the Chemical Agencies of Electricity.' Six years previously he had written, "Galvanism I have found, by numerous experiments, to be a process purely chemical." In the interim, water had been decomposed by electricity, and Davy began his researches with an inquiry into the changes produced in water by electricity. His main conclusion was that "the kind of polarity of each element determined the electrical and chemical actions shown by it." The French Academy awarded him a medal for this work; and from these discoveries the fame of our laboratories took its rise.

The next year Davy began a new series of experiments on Polarity. He exposed different substances to the action of platinum wires coming from a battery of 100 cells; and on October 6 he wrote in his note-book, "Remarkable phenomena with potash." On the 19th he made the following entry, "A capital experiment proving the decomposition of potash." He worked at the decomposition of other alkalies until the 23rd November, when he was attacked by a fever which proved nearly fatal to him.

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* The King at this time had his flock of merino sheep, and Sir Joseph Banks had the care of them at Kew. On his recovery from his first mental attack the King would only call the P. R. S. his woolstapler.

The importance of these decompositions to the recent science of spectral analysis, although not dreamt of at the time, can hardly be overrated; and I will therefore venture to interrupt my narrative for a moment by an experiment,—a very well-known one, with a slight modification, which will serve to illustrate the point. [The speaker then exhibited the dark absorption-line of sodium; but so arranged as to show the dark line *in the centre of*, and not entirely obliterating, the bright line; proving that a certain density of vapour is necessary for complete absorption.]

In 1808 Davy began to work on the composition of muriatic acid; and with a new battery, provided for him by subscription, he attacked different substances with increased energy. In 1810 he sent to the Royal Society his researches on oxymuriatic acid and the elements of muriatic acid, on what is in fact now known as chlorine.

In 1811 he made the acquaintance of Mrs. Appreece, and in 1812 wrote to his brother, "In a few weeks I shall be able to return to my habits of study and research. I am going to be married to-morrow, and have a fair prospect of happiness with the most amiable and intellectual woman I have ever known." The issue of these hopes has been written by his biographers; but the disappointment of the last seventeen years of his life is illuminated by the invention, not less original in its conception than benevolent in its object, of the Safety Lamp.

The great value of this contrivance, and of questions arising out of it, will, I trust, be sufficient apology for diverging again from my story in order to mention some very important experiments now in progress by Mr. Galloway. Explosions, it is well known, occur even in cases where the safety lamp is used. And it has been noticed that in these cases they occur most frequently after the firing of a blasting shot in the neighbourhood; and as it was almost certain that the penetration of the flame through the gauze of the lamp was not due to a sudden flow of gas from one part of the mine to another, experiments have been instituted to determine whether the transmission of the sound wave, or wave of compression, may not have been the means of producing the mischief. Through the kindness of Mr. Galloway we have here a tube arranged for making such an experiment. At one end there is the inflammable current enveloping a safety lamp; in the centre is a loose diaphragm, and at the other end a pistol will be fired, by the explosion of which a sound wave will be propagated along the tube. On the arrival of the sound wave at the extremity of the tube, the combustion will pass from the inside to the outside of the safety lamp. But I here leave the matter in the hands of Mr. Galloway, of whose experiments we hope to hear more hereafter.

Of the next great name connected with our Institution, namely, Michael Faraday, of his life and his discoveries the history has been already written, so far indeed as it can be written, by Bence Jones, by Tyndall, and by Gladstone. Si monumentum quæris circumspice.

These volumes of notes, from 1831 to 1856, will give some idea of the amount of work which he did in our laboratory; and their value will be better appreciated through the consideration that before these notes were made, no less than sixty of his scientific papers had been printed, nine of them in the 'Philosophical Transactions.'

Those of us who were present at Tyndall's two memorable lectures on 'Faraday as a Discoverer,' are not likely to forget the impression of the man left by them on our minds; and for those who were not present, it would be an office thankless to your lecturer and burdensome to his hearers, to contribute a feeble reproduction of those life-like memoirs. For our present purpose it will be sufficient to say that the entire fabric of those brilliant and manifold contributions to human knowledge were wrought out within the walls of the Royal Institution.

His great experiments have been so often and so well exhibited in this theatre, that some apology is needed for bringing any of them before you again; but in repeating for my own instruction some of those which bear more particularly upon the subject of Light, I have been tempted to reproduce one of them here. In doing this I have been perhaps moved more by a fascination of the phenomenon, and by a piece of instrumental good fortune which enables me to introduce an old friend under a new garb, than by any better reason. The experiment in question is that which Faraday called "the magnetization of light, and the illumination of the lines of magnetic force;" we should now term it the rotation of the plane of polarization under the influence of the magnetic field. But in order that we may not even by inadvertence confuse the rotation here produced with that due to quartz, or oil of turpentine, I will draw your attention, by way of memorandum, to the nature of the magnetism produced by spiral currents in given directions, and of the rotations of free currents produced by magnets.

[The lecturer then showed the opposite rotations of two sparks discharged about the two poles respectively of an electro-magnet, and the reversal of those rotations, first by a change of the poles, and secondly by a reversal of the direction of the sparks.]

You now see upon the screen an image of the figures produced by a magnificent piece of heavy glass under the action of polarized light. Its size enables me to make use of about four times the amount of light usually available in this experiment; and I have taken advantage of the figure which its imperfect annealing produces, to vary the effect upon the screen. The dark parts of the figure indicate the parts of the beam in which the vibrations are perpendicular to those transmitted by either polarizer or analyzer, and which are consequently cut off. Now if anything should intervene to change the plane of those vibrations a portion of them will be transmitted, and a partial illumination of the screen will ensue. This turning of the plane of vibration is effected by the magnet as soon as its force is developed by the electric current sent through its coils.

[The lecturer then "dispersed" the dark lines of the figure by means of a plate of quartz; and after turning polarizer and analyzer so as to colour the centre of the field with the tint intermediate between red and violet (*teinte sensible*), he showed that when the magnet was excited the field was rendered red or green according to the direction of the poles.]

Professor Frankland before coming to us had isolated the compound radicals Methyl, Ethyl, and Amyl, and had proved their resemblance to Hydrogen. He had also combined them with the metals zinc, tin, and mercury, and with the metalloid boron. By this means he had obtained very powerful chemical reagents, which proved of eminent service in subsequent operations. An instance of this power will be found in zinc-ethyl, which by its rapid combination with oxygen of the air, bursts into spontaneous combustion as soon as a flask containing it is opened.

In conjunction with Mr. Duppa, Professor Frankland worked in our laboratory at the artificial formation of ethers. They treated acetic ether with sodium and with the iodides of methyl, ethyl, and amyl; and by their means they arrived at a method for the formation of many organic substances which had previously been obtained only through the agency of animals or of vegetables.

In 1866 Dr. Frankland determined by a long series of calorimetric experiments the maximum amount of force capable of being developed by given weights of the different foods commonly used by man.

In the following year he investigated the effect of pressure (up to 20 atmospheres) upon the luminosity of flames of hydrogen and of carbonic oxide. He found that these flames, so feebly luminous at ordinary atmospheric pressure, burn with brilliant light under pressures of from 10 to 20 atmospheres, and that the spectra of these brilliant flames are perfectly continuous. From the latter circumstance he inferred that solar light may be derived from glowing gas and not from incandescent solid or liquid matter.

As these researches have so important a bearing upon spectral analysis and solar physics, I will venture to repeat one or two of the experiments. Here are three closed tubes filled respectively with hydrogen, oxygen, and chlorine, at atmospheric pressure. The densities of these substances are in the proportions 1 : 16 : 35½; and if the spark from an induction coil be made to pass through them, the luminosity of the discharge will be found to increase with the density of the gas.

That this result is really due to the density, and not to the chemical constitution of the gases, may be proved by allowing the discharge to pass through this tube filled with air, and by pumping air into it during the discharge. It will then be seen that the brilliancy increases with the pressure.

These researches were suggested by an old experiment of Cavendish's, in which he exploded a mixture of oxygen and hydrogen, first under atmospheric pressure and then under a pressure of from 10 to 12 atmospheres. In the first case there is much noise and little light; in the second, a brilliant flash and no noise. The labours of Dr. Frankland have rendered this experiment intelligible, and have correlated it with other phenomena.

Of Faraday's successor, John Tyndall, I am greatly at a loss how to speak. In this place his presence seems so near to us, his thoughts so subtle, his words—even when rung back to us from those busy cities far away on the other side of the Atlantic—so familiar and yet so stirring, that it behoves us that ours should be wary and few. Few men have brought so large a burden and bulk of contribution to the common stock of knowledge; but still fewer have inspired in his hearers so strong a love, such ardent enthusiasm for the subjects of his research.

It is now twenty years since Professor Tyndall began his researches in our laboratory. During the first thirteen years he produced no less than thirteen papers, which were printed in the 'Philosophical Transactions': on Sound, on Diamagnetism, on Glaciers and Ice, on the Radiation and Absorption of Heat, and on Calorescence.

In these he established the important fact that if the various gases be arranged in order according to their power, first of radiating heat and secondly of absorbing radiant heat, the order will be the same in both cases. He further proved that the chief absorbing action of our atmosphere on non-luminous heat is due to its aqueous vapour. He applied his discovery to the explanation of many meteorological facts: *e.g.* the great daily range of the thermometer in dry climates; the production of frost at night in the Sahara; the cold in the table-lands of Asia, &c.

He discovered also the means of separating the invisible from the visible radiations, and proved that in the case of the electric light the former is no less than eight times as powerful as the latter. He also made the daring experiment of placing his eye at a focus of dark rays capable of heating platinum to redness.

Since 1866 his attention has been largely occupied in examining the action of heat of high refrangibility (instead of low), as an explorer of the molecular condition of matter.

In this investigation one obstacle to be overcome was the presence of the floating matter in the air. The processes of removal of these particles became the occasion of an independent research, branching out into various channels; on the one hand, it dealt with the very practical problem of the preservation of life among firemen exposed to heated smoke; and, on the other, it approached the recondite question of spontaneous generation.

He subjected the compound vapours of various substances to the action of a concentrated beam of light. The vapours were decomposed, and non-volatile products were formed. The decompositions always began with a blue cloud, which discharged perfectly polarized light at right angles to the beam. This suggested to him the origin of the blue colour of the sky; and as it showed the extraordinary amount of light that may be scattered by cloudy matter of extreme tenuity, he considered that it might be regarded as a suggestion towards explaining the nature of a comet's tail.

[The lecturer then exhibited the polarization of light scattered by small particles suspended in the medium traversed by a beam from the electric lamp, employing for the purpose the chromatic effects due to the circular polarization of quartz.]

His volume of contributions to molecular physics in the domain of radiant heat, which contains only his original investigations on this subject, would alone suffice to show what is doing in the laboratory of our Institution.

If we compare him to Faraday at the same time of life, he has still many years of intellectual energy, the conversion of which into its scientific equivalent may, perhaps, be effected within these walls.

No one has regretted the destruction of the laboratory of Davy and of Faraday more than Professor Tyndall. He almost prayed for the preservation of the place where their discoveries had been made; but as soon as he saw that in our struggle for existence such material aids as improved buildings would conduce alike to the progress of science and to the permanence of the Institution, he withdrew his objections, and threw all his powers into making the new laboratories as perfect as possible for the good of his successors.

I add a few words on the reasons which led the managers to recommend the rebuilding our laboratories, and the consequent demolition of the place where the great discoveries that I have touched upon were made. In the opinion of those best qualified to judge, our chemical laboratory was badly ventilated, badly lighted, badly drained, and quite unfit to be occupied for many hours daily. It was probably the very worst, and certainly all but the worst chemical laboratory in London; and compared with more modern ones both at home and abroad, it was nowhere. The physical laboratory had remained for nearly seventy years in its original state. At first it was said to be equal to any laboratory; but then there were hardly any in existence in this country; and during the last few years such splendid edifices have arisen in London, in Oxford, in Cambridge, in Manchester and in Glasgow, and elsewhere, that the laboratory of Davy, of Faraday, and of Tyndall was much inferior to the private laboratories of the professors who carry on their course of instruction in public rooms of still greater size and extent of resource. The main purpose of our laboratories is research, and instead of offering by their excellence an inducement to professors to come and to stay, the one was a mere makeshift, the other a noble relic. Neither offered facilities which were not offered in a larger measure elsewhere. And those only who know what is going on both at home and abroad can form an adequate idea of the competition which, in this respect alone, will prevail for a generation to come.

By the construction of new laboratories this material disadvantage will be removed. Future professors will have buildings constructed to aid research. Your liberality has spared no judicious expense; and, so far as the site would admit, our laboratories will be as perfect as the skill of our architect and the advice of our professors can make them.

In conclusion, let me lay before you what must still be done, in order that there may be earned for the new laboratories a reputation comparable with that which has hitherto proved both our glory and our support.

Our first and foremost object, beyond bricks and mortar, and money, and apparatus, must be to find a succession of professors of the old type; men who love research. But even Faraday would perhaps have been compelled to leave us, on account of the smallness of the sum which we could afford him, had not the endowment of the chemical chair, with 100*l.* a year, by the late Mr. Fuller, happily intervened. This timely endowment was probably a critical turning-point in the history of the Institution. We may not easily find successors worthy of the great names who have gone before them; but we may do much toward preventing mistakes in future appointments by keeping steadily in view, that the promotion of natural knowledge is our main object; and that instruction and amusement, and brilliant audiences are all secondary to our principal purpose. Not that these subsidiary purposes are to be neglected or despised;—and I, as your Treasurer, should be the last to undervalue them, but we feel confident that if the main purpose is effected, all the others will follow as a simple sequence.

Secondly, when we have found professors of the type that I have described, our next need is that we may be able, from independent resources at the disposal of the Institution, to offer them a remuneration which, all things taken into account, shall be an equivalent to what they would receive elsewhere. So that neither Government appointments, nor University professorships, nor the liberality of mercantile men, should be able to lure them from the path of discovery, to tuition, to arts, or to manufactures.

The one act of wisdom, among the many aberrations of an eccentric Member of Parliament, saved Faraday to us, and thereby, as seems probable, our Institution to the country. The liberality of a Hebrew toy-dealer* in the East of London has made the rebuilding of our laboratories possible.

It is said that Mr. Fuller, the feebleness of whose constitution denied him at all other times and places the rest necessary for health, could always find repose and even quiet slumber amid the murmuring lectures of the Royal Institution; and that, in gratitude for the peaceful hours thus snatched from an otherwise restless life, he bequeathed to us his magnificent legacy of 10,000*l.* If this evening's discourse shall have ensured one such blissful hour to any of his audience, your lecturer's efforts will not have been altogether in vain. But to each such happy individual he would express the hope that, as you have resembled Mr. Fuller in your experience of life, so may you emulate him in your liberality at death. In short, I would conclude almost in the words of old Bishop Andrews: Unum operæ meæ pretium abs te peto, hoc autem vehementer expeto, ut mei peccatoris meorumque in precibus interdum memor sis. Which being interpreted is:

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* Mr. Alfred Davis, after paying his composition of sixty guineas, as a Member of the Institution, and three annual donations of twenty guineas for the promotion of research, at his death in 1870 bequeathed 2000*l.* for the same purpose. His deafness prevented him from deriving any benefit from the Lectures.

For these my efforts I beg but one thing in return, and this I beg most earnestly, *viz.* that you will now and then remember me a sinner against your patience and forbearance in your prayers, and that you will also be mindful of our professorships in your wills.

[W. S.]

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The following Table of the principal items of original work done by our Professors, taken in connection with their long series of laboratory notes, forms a monument of the intellectual activity, the manual dexterity, and the persevering industry, developed in the Laboratories of the Royal Institution:—

## DAVY.

| | |
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| 1806 | Chemical Agencies of Electricity. |
| 1807 | Decomposition of Potash. |
| 1810 | Chlorine. |
| 1812 | Discourse on Radiant or Ethereal Matter. |
| 1813 | Iodine. |
| 1815–6 | Researches on Fire-damp and Flame. |
| 1817 | The Safety Lamp. |

## FARADAY.

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| 1820 | Alloys of Steel. |
| 1821 | History of Electro-magnetism. |
| „ | Magnetic Rotations. |
| 1823 | Liquefaction of Chlorine and other Gases. |
| 1825–6 | New Compounds of Carbon and Hydrogen. |
| 1825–9 | Manufacture of Optical Glass. |
| 1831 | Vibrating surfaces. |
| „ | Magneto-Electricity. |
| 1832 | Terrestrial Magneto-Electric Induction. |
| 1833 | Identity of Electricities. |
| 1834 | Electro-Chemical Decomposition. |
| „ | Electricity of the Voltaic Pile. |
| 1835 | The Extra Current. |
| 1837–8 | Frictional Electricity. |

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| 1837–8 | Specific Inductive Capacity. |
| 1845–8 | Magnetization of Light. |
| „ | Lines of Magnetic Force. |
| „ | Magnetic Condition of all Matter. |
| „ | Diamagnetism. |
| „ | Magne-Crystallic Action. |
| 1849–50 | Magnetism of Flame and Gases. |
| „ | Atmospheric Magnetism. |
| 1856 | Relations of Gold and other Metals to Light. |
| 1860 | The Regelation of Ice. |

## TYNDALL.

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| 1853 | Transmission of Heat through Organic Substances. |
| 1854 | Vibrations due to Contact of Bodies at Different Temperatures. |
| 1855 | Researches on Diamagnetic Force. |
| 1856 | Slaty Cleavage. |
| 1857–8 | Physical Properties of Ice and Glaciers. |
| 1859–63 | Absorption and Radiation of Heat by Gases. |
| 1865 | Calorescence. |
| 1866–7 | Action of Heat of high Refrangibility. |
| 1868–9 | Formation of Clouds. |
| „ | Colour and Polarization of the Sky. |
| 1870 | Smoke and Dust Respirator. |

## FRANKLAND.

| | |
|---|---|
| 1863–6 | Synthesis of Acids of the Lactic Series. |
| 1863 | Mercury-methyl, Mercury-ethyl, and Mercury-amyl. |
| 1864 | Transformation of Organo-Mercury Compounds into Organo-Zinc Compounds. |
| „ | Combustion of Iron in Compressed Oxygen. |
| 1865 | Synthesis of Acids of the Acrylic Series. |
| „ | Synthesis of Fatty Acids. |
| 1866 | New Organic Radical Oxatyl. |
| „ | The Source of Muscular Power. Potential Energy in various kinds of Food. |
| 1867 | Source of Light in Flame. Effect of Pressure upon Luminosity of Flame. |

## WEEKLY EVENING MEETING,

Friday, Jan. 24, 1873.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

The Rev. Thomas Rawson Birks, M.A.
PROFESSOR OF MORAL PHILOSOPHY, CAMBRIDGE.

### *On the Analogies of Physical and Moral Science.*

The Analogies, on which I would offer a few remarks this evening, are of two kinds—general and specific. The former are those which depend on the nature of the subjects in themselves. The latter are correspondencies, by which the Maker of both worlds, the natural and the moral, seems to have stored all outward nature with countless parables, by which to lead our dull thoughts upward to the things unseen and eternal.

The first general analogy between Physics and Moral Science is in the foundation on which they rest. In either case some first principles must be assumed. We cannot prove everything. A demonstration requires premises. You cannot reason on colours with one who is wholly blind from his birth. You cannot reason on right and wrong with one whose conscience is wholly dead. There must be intuitions of space, before there can be a science of geometry. There must be perceptions of force and power, before there can be a science of dynamics. There must be a sense of right and wrong, before there can be a Science of Morals, and an instinct of religious reverence, and uplooking to an Unseen Source of moral perfection, before there can be a true Theology.

The δὸς ποῦ στῶ of Archimedes is as needful in Physics as in Morals. No lever can act without its fulcrum. No conclusions can be reasoned from experiment, or from the processes of geometry, until we accept, as trustworthy, the evidence of our senses, the testimony of honest witnesses, and the veracity of our own powers of thought and reason.

Again, Physics and Morals agree further in this, that the first principles on which they build rest on a basis of deep, unexplained mystery. A violent attempt has been made to clear physical science from those troublesome, mischievous cobwebs, metaphysics and theology. But in striving to sweep them away, science itself is destroyed. Phenomena, not referred to causes or forces, cannot even be grouped together. Nay, a phenomenon is something appearing to some one. If there be no *thing* that appears, and no *person* or percipient to whom it appears, there is no phenomenon. But what is a person? What is a thing? They are metaphysical ideas. Arithmetic starts from the unit. But what is a unit? The One and the Many has been a metaphysical puzzle from the days of Parmenides. In what does individuality consist? A line is one line, but it has two sides and infinite points. A person is one person. But he has many faculties, and thoughts innumerable. Geometry starts with definitions of a point, a line, and a surface. Here are as many mysteries as words. They all belong to space. But what is space? a thing or an attribute? Is it finite or infinite? Why are there three dimensions, and no more? How can there be a point without size, a line without breadth or thickness? And when we pass on to dynamics, the difficulties multiply. Momentum, *vis viva*, accelerating force, velocity and time, a deep, unsearchable mystery lies couched and latent in every phrase.

In algebra, however mechanical its processes may seem, look below every symbol, and you will find a mystery concealed. Take the simple formula for the area of a circle, $\pi r^2 = a$. The sign of equality is one mystery. For what is it for two things to be equal and not the same? Equal numbers are the same number, but equal lines are different, and not the same. The idea runs up into morals and theology. What are equal rights and equal laws? In what sense are the ways of God equal, the ways of sinful men unequal? The symbol $a$ denotes an area, the symbol $r$ a radius or line. Can a line multiply a line, to make an area? Or if they are numbers, can we deal with infinite incommensurables as with finite numbers? The number $\pi$ is a mystery. It represents a definite ratio, and yet one which no finite fraction can express. The series of integers forms the basis of the science of arithmetic. A circle is one of the simplest figures in geometry. But when we would pass from what is simplest in one science to what is simplest in the other, the infinite steps in, and blocks our path. We can approximate as near as we please. We can detect a hundred infinite series to which $\pi$ is equal, but a finite expression for it we cannot ever obtain.

The case is similar in the higher region of morals and religious faith. The difficulty is neither more nor less, but precisely the same. Man is a free agent, responsible for his actions. This is a first principle in morals. Man is dependent largely, in his feelings and instincts, on the conditions of his animal nature. This is a first principle in human physiology. Man, to fulfil his duty and act aright, needs the offered help of Divine grace. This is a first principle of Christian Theology. But who shall explain perfectly the union and harmony of these three elements? The first, divorced from the others, will create a proud, self-flattering Stoic; the second, a lazy sensualist; the third, a superstitious, immoral mystic. In their union alone heroic virtue is reconciled with practical common sense, and religious faith and reverence. But, just as in squaring the circle, truths simple, but different in kind, may need a passage through the infinite, and thus exceed the range of our finite minds, in order to reconcile them and reveal their perfect harmony.

The ultimate scientific, and the ultimate moral and religious ideas, are alike mysterious. Those who, on this ground, would deny moral science and religious truth, must be content to see geometry, the doctrine of space, and dynamics, the doctrine of force, involved in the common ruin. He who refuses to see by any light which has a haze of mystery around it, or dark line contained in it, must close his eyes even to the sunlight, and remain in darkness for ever.

Again, Physical Science has a twofold character. It deals with truths and with facts, truths of reason, and facts of experience, and its triumph is in their marriage. Set aside the deductions of abstract science, and natural philosophy sinks into a lumber-room of countless observations, unreduced and unexplained. Neglect experiment and observation, and pursue abstract theories alone, and it resolves itself into a fairyland of chimeras and empty dreams.

The character of Moral Science is the same. It has its truths and its facts, which must not be confounded together. Its truths are the laws of eternal righteousness, which will not bend, to humour the weakness, or disguise the perverseness, of erring and sinful men. These abide the same in their grand outlines for ever, and include all actual and all possible moral agents within their firm and mighty empire. Its facts are the special conditions of human life, the strange complexities which arise from man's animal and spiritual nature, and the lessons of his actual moral state, his virtues or vices, his capability of brutish debasement, and his capacity of lofty faith and moral progress, derived from observation on the real history of mankind. And here, too, there is the same double danger. Shrewdness of observation, and keen insight into human character, can never create a moral science worthy the name, without the clear vision of high and eternal truth; while abstract disquisitions on the beauty of virtue, and eternal and immutable laws of right, will remain almost wholly useless, like sounding brass or tinkling cymbal, unless they are brought into contact with the realities of moral conflict and temptation, and reconciled with them through the condescending messages of redeeming love.

The analogy of Physics and Morals in their growth and progress, and their common hope of further advances to be made, and fuller insight to be attained, may perhaps be veiled from superficial observers. But there is a resemblance of an opposite kind, which it seems impossible to overlook or deny. The advance of knowledge, in the field of Physics, is great and manifest; in that of Morals, it is real and steady, though less apparent. But in each alike our remaining ignorance is far greater still. They both, when seen aright, teach us the same lesson of deep humility. In the lower field of thought as in the higher, the words of Newton are still true. We are only children, picking up shells, or perhaps even jewels, on the shore while the great ocean of truth lies undiscovered before us.

Where can we turn in the fields of Physical Science, and not feel, after all our boasted progress, that we are infants in knowledge, and far more still remains unknown? The law of gravitation is the chief triumph of modern research. Its results have been deduced by profound analysts, for the moon, the planets, the satellites, and the comets and the double stars, through two hundred years, and have yielded a rich harvest of discovery. Yet we cannot solve, even now, the problem of three bodies only, except in special cases, and by successive approximation. But we have to assume that every star and planet, nay, every atom composing them, is fulfilling this law with unfailing accuracy, when acted upon every moment by thousands of stars or planets, and by the billions or trillions of atoms that compose them. Is, then, each atom omniscient? Does it know, by a sure instinct, the distance of every other atom in the universe to a decimal of a thousand places, and govern its own movements by the resultant of the whole? If so, each particle of matter must be infinitely wiser in the reckoning of distance, and in the summation of ever-varying quantities, than the ablest mathematician of past or present days. Does each atom act instantaneously at every point, through a sphere which reaches to the farthest and dimmest star? Then it would seem that it acts everywhere but where it is, and must be virtually omnipresent. The infant's hand, uplifted in idle play, affects the motion of Sirius. The dew-drop that falls unseen in the night, the tear on the cheek of sorrow, is carried downward by the help of every wave that dashes on the shores of New Zealand, and by every atom in those deep places of the earth which science has never explored. It acts in turn with a measurable force, however small, on every star in the midnight heavens. Science might advance at its present rate for millions on millions of years, and still would fall infinitely short of reckoning up all the consequences of this one law of nature, so beautifully simple, so inexpressibly mysterious and profound!

The lesson of Moral Science is the same. Some laws of duty are plain and clear to the humblest and the least intelligent. But when we pass beyond the first elements, we meet with difficulty on every side. The laws of outward nature, the free will of man, the Divine sovereignty, with its promises of grace, who can integrate the vast moral equation, which includes these three grand elements, or trace out the countless results that must flow from it in the ever-changing drama of human life? We may see dimly that all the elements of the moral world, like those of the natural, must act and react on each other. But when once we step beyond the plainest laws of duty, or the simplest lessons of revealed religion, mystery shuts us in on every side. Every thoughtful mind is compelled to exclaim, "O the depth of the riches both of the wisdom and knowledge of God!"—the Governor of this vast universe of moral agents!—"How unsearchable are His judgments, and His ways past finding out!" Pride ill befits the students of science, whether in its lower or its higher field. If they have wings, with which to traverse swiftly the starry depths of space, they need surely to have others, with which to veil their faces while they adore.

From these general analogies of Physical and Moral Science, I pass on to others of a more specific kind.

And first, the doctrine of atoms is one of the chief features of our modern chemistry, including its kindred science of molecular forces. Scientific analysis divides and subdivides, and comes at length to a limit which it cannot pass. The chemical elements, each with its own combining number, point to the real existence of atoms, having their proportional weight, which build up the structure of all solids by their union. Some way or other, the properties of the aggregates depend plainly on their atomic elements. These determine their forms of crystallization, their density, their optical, magnetic, and electric powers. A clearer knowledge of their real nature would introduce a new era, more striking than all the past, in the progress of Physical Science.

In Moral Science, also, there is a great and simple fact, which answers, by close resemblance, to this atomic theory of modern chemistry. This is the individual personal responsibility of each moral agent.

Human society is a very complex whole. The man seems often almost lost amidst the millions of a civilized nation, the statistics of its trade and commerce, the tens of thousands of its armies, and the grand developments of its social and political life. But a nation is not like a cube or surface, capable of endless subdivision. Analyze it aright, and we soon reach the moral units of which it is composed. These men, women, children, are endued with moral characters, and each is separately responsible to the others and to their God. You cannot resolve such an agent into decimal fractions. You cannot fuse man down into some composite mass, in which his own responsibility shall disappear.

Material atoms are elastic. Force cannot destroy them. Compression, however violent, calls out those immense latent powers which were concealed in them before. And it is just the same in the moral world. Tyranny leads to a strong reaction. The conscience repels the usurpations of lawless force, whether issuing from an Eastern despot, or some fierce and wilful democracy; and asserts its powers anew, whether in martyrs at the stake and patriots dying for their country's liberty in the battle-field.

The dominion of law is another great fact, which has revealed itself more and more clearly in the whole progress of modern science. With some minds it seems ready to swallow up every other. Laws, which can dispense with a Law giver, and create their own subjects, are a paradox of some modern philosophers, but unknown and without any example in the social world, where the idea has its natural home. The true meaning of the phrase, in physics, is that changes result from forces, with which the particles of bodies, or living things, are endued; and that these forces, in a large number of cases, are measurable, and vary in their amount by some rule which can be discovered. The term, however, may be extended to those truths of arithmetic and geometry, which co-exist with the whole series of physical change, and which we feel to be necessary, and their opposites inconceivable. We have thus, speaking strictly, a double principle or fact of science, the permanence of unalterable truth, and the constancy of laws that are not necessary, which might be conceived different, and are proved by the inductions of experience alone.

Is there nothing in the field of morals which answers to this main feature of modern physical science? The correspondence is exact and entire. First, we have a large class of moral truths, which answer to those of arithmetic and geometry; only that they deal with higher and nobler subjects, not with abstract units, or points and lines in space, but moral agents, and their relation to each other, and to the great Source and Fountain of their being. It is just as easy to conceive that two right lines can enclose a finite space, as that it is morally right for a man to hate all his fellow-men, and to think the properties of a circle may be reversed and turned into those of a mangle, as for treachery to become a virtue, and gratitude a crime. But besides moral truths, unalterable in themselves, our reason recognizes laws of another kind, dependent on the will of the Creator, and the actual constitution of the moral universe. Such, for instance, are the force of example, the strength of sympathy, and the power of habit. Such are all the ordinances of marriage and of property, and the various relations which grow out of the actual constitution of men, in whom an animal nature and a higher life are mingled strangely together.

Again, the law of universal attraction is the boast and glory of modern physical discovery. No other physical truth has been so wide in its range, or so wonderful in the triumphs of mathematical reasoning, and practical and assiduous observation, to which it has led. Has this no counterpart in the higher sphere of morals? There is one of the simplest and plainest kind. The great law of duty, "Thou shalt love thy neighbour as thyself," binds together, with a Divine bond, the whole moral universe. It reaches to the highest, and stoops to the lowest. It includes every being, on earth or in heaven, or in unknown worlds, who is endued with the power of choice and will. It speaks to the conscience of the child, trained in Christian faith at its mother's knee, and claims the allegiance of the noblest and loftiest of those spirits which stand before the eternal throne. Like the great law of which it is the moral counterpart, it

> "Lives through all life, extends through all extent,
> Spreads undivided, operates unspent,"

and exercises a powerful control of binding, irreversible authority throughout the vast range of the moral universe.

But universal attraction, though it claims historical precedence, is not the only law which obtains in the wide world of natural philosophy. There is a moral law of molecular repulsion, towards which modern science, with slow and painful steps, seems to be groping its way. It has long seemed to be plainly implied in the intense elasticity of the waves of light. It has been still more clearly revealed, of late, by the strange phenomena of Halley's comet and several others, in their near approach to the sun. In the words of Sir J. Herschel, the force displayed is "repulsive as respects the sun, and far exceeds in energy the force of gravitation." When once its nature is more clearly seen and defined, we may expect an advance of physical science still greater than has been achieved since the immortal 'Principia' appeared.

Here, again, there is a moral counterpart of the plainest kind. There is, in the innermost sphere of thought, a principle of self-assertion and personal responsibility, without which the social instinct itself would be fatal to moral life; and, under the sway of numbers and the tyranny of majorities, all power of moral action would expire. Side by side, also, with the grand law of love, which binds together the moral universe, there is a further law, second only to the first, of purity and holiness, by which the upright and pure repel from themselves the pressure and threatening invasion of all moral evil. And, just as in the natural world, so in the moral, the law of repulsion, compared with the force of attraction, is less easy to define, more complex and mysterious in its working; but is found, by those who look below the surface, to be no less essential to the harmony and well-being of the whole.

Another striking feature in the discoveries of modern physics, is the law of order and mutual dependence which they reveal. Let us look at the solar system, as unfolded by astronomy. It is severed, first of all, by immense intervals, from the nearest stars. It forms one mighty whole. The sun, in its centre, exceeds a thousandfold the collective mass of all the planets that circle around it. The planets, in their turn, exceed their own satellites, and hold them in due subordination. They vary in size themselves, from Jupiter, a thousand times larger than the earth, to Mercury twenty times, and to asteroids a thousandfold less. Each planet, again, has its own atmosphere. The sun has his crown of glory and his nebula of fainter light, far diffused; the earth its gentler auroral displays, and the superior planets give some signs of being partly self-luminous. Everywhere there is a marked subordination, and on this fact the possibility of a scientific calculation of their orbits mainly depends.

The same principle prevails clearly in the moral world. There are common laws of moral duty and obligation, and of creature dependence, which belong equally to all. But in the actual state, the various gifts and privileges of moral agents, there is a wide and immense diversity. In atoms there is diversity of weight, from hydrogen to gold. In planets, diversity of size, from the tiny asteroid that eludes the lynx-eyed observer, to the lustrous Jupiter, with its belts and satellites, and a circumference of near three hundred thousand miles. In the discoveries of science, as well as in the first impressions of the eye, one star differs from another star in glory. In plants there is vast disparity, from the cedar of Lebanon to the hyssop on the wall, or the algæ of the microscope; and in living organisms, from the huge elephant or extinct mammoth, to the minute forms of life which the same microscope alone reveals. And in the higher range of moral being it is the same. Equality is a chimera of mere dreamers, and a dead, monotonous level can nowhere be found. There is diversity of experience, from the feebleness and ignorance of infancy, to the ripe wisdom of age. There is diversity of rank, from the pauper, dependent on the alms of others, to the monarch on the throne. There is diversity of mental power, from the helpless idiot to the poet or philosopher who explores the secrets of the universe. There is diversity of moral condition, from the wretched slave of brutish appetite to the noble army of martyrs, and the goodly fellowship of prophets, whose lips and hearts are touched with fire from heaven.

Men are found to tell us, in these days, that society tends irresistibly to a dead level of pure and absolute democracy, where genius, rank, character, virtue, shall count for nothing, and the power of numbers alone shall prevail. They might as well persuade us that the wheels of time are rolling backward, and the universe is resolving itself into nebulous mist again. All nature, with one voice, points clearly to an opposite view. If an old social order has been dissolved, from time to time, like a solid which first melts and then evaporates, it is that some new precipitate may be formed, perhaps one more crystalline in structure, and thus more pervious to the light of heaven. In the moral world, as in the natural, a dynasty of homogeneous atoms, with no polarity and no bonds of special cohesion, can never last. Wherever there is life, there must be organization. And all members have not the same office. The least and lowest may have its own use and honour, but can never, without shame and mischief, usurp the place of the higher. Least of all can even the noblest creatures, without the risk of some fatal loss and moral catastrophe, attempt to shake off their subjection to that Fountain of Life, and Sun of the moral universe, on whom they all depend.

The power of nearness or proximity is another great fact, which pervades the world of physics. On this, in some way or other, cohesive action plainly depends. It is seen even in the law of gravitation. It seems to operate, among the atoms of bodies, in a still intenser form. And is it not just the same in the world of morals? The great command, "Thou shalt love thy neighbour as thyself," teaches clearly that neighbourhood is to form one main element in the actings of moral duty and Christian love. All are to be loved. But the forthgoings of affection, like the circles formed by the pebble in the lake, are to be strongest and fullest towards those who dwell at our side. Destroy cohesive force in adjoining atoms, and the universe would be unmade, and suns and planets become nebulous mist once more. Destroy the family affections, and social life must equally perish and pass away.

But there is another principle, the contrast and complement of the last, which the facts of astronomy, and indeed of all physical science, clearly reveal — the mighty power of Union. Forces, separately feeble and insignificant, when grouped together, and formed into one coherent mass, have an influence they can never obtain while disunited and incoherent. The cohesion and mass of the comets is small. We have seen one of them part asunder. Perhaps we witnessed, the other day, a brilliant shower of its comminuted fragments, spending themselves in our atmosphere, and working no sensible change. They pass near to the planets and satellites, and no disturbance of their motion follows. But let the same elements be combined in a greater mass, and they become a planet, maintaining its own orbit, and sustaining its own forms of life. Let the mass be further increased, and we have a central sun, the source of light, heat, and order to the mighty system over which it rules.

The very same principle obtains in the higher world of morals and intelligence. Union is strength. It is not strength alone, but light and heat also. The combination of myriads and millions has a mighty power. But they must first cohere, or this power of union is unattainable. Truth, faith, and love, are the secret bonds by which alone they can be united in a permanent and effectual way. Common selfishness, or some tide-wave of popular passion, may combine them for a moment, but only to form an explosive mixture, soon to be scattered violently again. But union, built up by truth and love, such as the union of the Christian Church and every Christian nation ought to be, though, alas! too little attained, has immense and mighty power. Once let the world witness a truly righteous nation, where truth fills every mind, and love every heart, and a common faith pulsates through the whole; and it would be like the presence of a moral sun, shedding light and warmth through all surrounding nations, and by its moral momentum would bind all the tribes and nations of the earth, like planets, into order and perfect harmony.

The analogies are no less striking which result from the whole recent course of optical science.

The prism, of late, has been unusually rich in new discoveries. The pathway in which Newton took the first main step, has been explored anew, and secret marvels have been disclosed in every step of the progress. These waves of light, from forty to sixty thousand in one inch, these swift undulations, hundreds of millions of millions in one second, baffle and confound us by their subtilty and strangeness. This beautiful gradation of tint and shade, derived from the pure white of the sunbeam, this strange fusion with heat at one end of the scale, and passage into magnetic force at the other, these dark lines that take their stations, like sentinels, in the midst of light itself, and turn, in other cases, into lines of double brightness, all stimulate the curiosity of science, and open a wide field for conjecture, while they delight the eye with their variegated beauty.

These facts, also, have their moral counterpart. "The Bow of the covenant" is an accepted symbol from the opening to the close of the book of God. And its double lesson is plain. The light of pure and perfect goodness needs to assume a varied colouring, and is refracted into many forms, of gentleness and severity, of justice and grace, the tints of the ruby and the emerald, when it shines through the storm-clouds of our present world. And the same is true of moral excellence in the creature, the image and reflexion of the Divine. It admits of strong contrasts and large varieties. There is the stern, heroic grandeur of the ancient prophets, and the weeping gentleness of a Mary Magdalene. There are forms of goodness which resemble fiery flame, that warms as well as enlightens, but consumes the stubble, and others that resemble rather the gentler green of the meadow, or the pure azure of the sky.

May we not carry the resemblance still further, even into the region of these new discoveries? There are dark lines even in the brightest parts of Divine Providence, interspersed amidst the purest and most attractive messages of the Christian revelation. They are there, whether we observe them or not. They are there, even when we cannot explain them. The sun sends us his message. It contains dark hieroglyphics, which we do not understand. Yet Science accepts them, and profits by them; and the docility with which it notes and receives facts it cannot yet interpret is the pledge and inlet of new discoveries. If we can detect and then measure the approach or recession of fixed stars, millions of miles distant, a problem that might seem impossible, it is not by light itself, but by darkness surrounded with light, and seen in the midst of it, that the triumph is won. There are also dark lines in Divine Providence, even where the light shines clearest. There are deep mysteries enshrined in that message of Divine love, which brings light to the mind of the Christian and warmth to his heart. And the docility with which we receive the whole spectrum of Divine truth, even when its strange, dark lines are unsolved mysteries, is perhaps the condition on which the highest attainments of Christian wisdom and true moral insight may depend.

But I must hasten to a close, though I have merely skimmed the surface of a vast ocean, and its deeper soundings remain wholly unexplored. All nature is one storehouse of parables to the thoughtful mind. Science, even when most careless, can hardly help stumbling on some of them in its way. But the more carefully we weigh its discoveries, the richer we shall find them to be in lessons of wisdom. The links which bind the planets to their sun are not so firm as those which bind this outward world of sense and matter to the higher and nobler truths of the spiritual world beyond and above. Nature is one vast mirror, in which we may see the dim reflexion of a nobler field of thought than the conflict of puny atoms, or integrals of atomic force, can ever supply. We need first to gaze downward, that presently we may look upward. Then turning from the shadows to the substance, from things seen and temporal to things unseen and eternal, we shall veil our faces before the vision of a greatness that is unsearchable, and a goodness that is unspeakable. In the spirit of Christian faith and hope, we shall thus gaze on the uncreated Light, and rejoice with humility, while we adore.

[T. R. B.]

## WEEKLY EVENING MEETING,

Friday, January 31, 1873.

Sir Henry Holland, Bart. M.D. F.R.S. President, in the Chair.

Edward Dannreuther, Esq.

### *On the Music of the Future.*

Possibly some of you may have dabbled a little here and there in the study of Harmony. If it has happened to you to make experiments with two or more masters, you will in all likelihood have been astounded to observe how much "the doctors differ." You will have been amused to see how congenial it is to musical wiseacres to carry on a war of "omnes contra omnes." You will have found different principles and different applications even in rudimentary matters. When the professors came to explain things a little more complicated than a hymn tune, when they came to account for the doings of artists of original genius, say of men such as Chopin, Berlioz, or Liszt, they are certain to have pronounced them wanting in clearness and perspicuity, in grammatical precision, and logical development of thought. That which the professor's critical yard of tape can neither measure nor encompass is to him a nonentity.

Like Newman Noggs's barber, every professor thinks himself bound to draw the line somewhere; say before the last works of Beethoven, or before Schumann or Chopin. He stigmatizes everything that extends beyond his little horizon of personal vision as madness, or at least as eccentricity; and the party-cries of madman, fanatic, barbarous destroyer of the beautiful, have been heard probably ever since the children of Jubal "handled the harp and organ."

Now the fact is, that in music, as in all other arts, the *practice* of great artists always precedes theory. In our day, I should say, practice is in advance of theory by about a quarter of a century; in Beethoven's time it was ahead by about fifty years, and before that by full a hundred. An artist of genius widens the scope and enlarges the domains of his art, and the critical and theoretical surveyor comes after to take his measure with a yard of tape.

It is near upon five-and-twenty years since Richard Wagner and Franz Liszt accomplished some of their most important revolutionary deeds in the realms of instrumental and dramatic music, and the time has come when musical theorists are following in the wake of the great innovators, and are exerting themselves to give a systematic account of their doings. Before attempting to give you some insight into the many conflicting theories, opinions, and absurdities, that have crystallized around that great bugbear, "The Music of the Future," it will be well to give you a slight notion of the state of musical matters antecedent to Wagner and Liszt's revolt. It will be best to do this from two sides: The side of pure instrumental music, which, as you know, has been far the most prominent in Germany (that is to say, in the entire musical world worth speaking of), since the middle of the last century; and secondly, from the side of music in connection with the Drama, that is to say, operatic music generally. Since the days of Bach and Handel, the progress of musical art has lain almost exclusively within the domains of purely instrumental music. It is true, that our great masters have produced an enormous amount of matter for human voices, both with and without accompaniment, and it is not less true that the art, even the purely instrumental part of it, has received an immense impulse through its connection with the stage. Still I may assert, that the achievements of the great instrumental school, from Bach and his sons, through Haydn, Mozart, and Beethoven, to Schubert, Mendelssohn, and Schumann, constitute the genuine triumphs of the art, without fear of injuring Rossini, and all his friends who have devoted their talents to the manufacture of tunes for Italian and French singing-birds. If we follow the course of the development of instrumental music from Haydn to Beethoven, we perceive a gradual development of form; from a regular production for the market, to the achievement of a lofty ideal, upon which none but purely artistic considerations are allowed to have any influence. In Haydn's earlier days, the greater number of Austrian, Bohemian, and Hungarian noblemen kept their private bands. A similar custom has obtained in Russia up till the recent emancipation of serfs, and may be still in vogue here and there for aught I know. Those who were unable to support a complete band, kept at least a string quartette, or a couple of wind-instrument players. "Pater familias" played the violin, or the flute, or the violoncello together with his retainers, and accompanied his daughters playing on the cembalo. It was for this sort of amateur gatherings that composers produced the incredible mass of chamber music, which astonishes the student. It is to these *réunions* of dilettanti, that the enormous number of duets, trios, quartettes, for string and wind instruments; the endless sets of variations for the pianoforte; the serenades, cassaziones, suites for small bands, &c., resembling one another like eggs in a basket, owe their origin. The composers furnished what their patrons could play easily; and thus a conventional form and style for compositions of this character was easily developed, and soon carried to a state of relative perfection.

But the very great diffusion of this class of music, and the ever-increasing demand for it, brought into play an element, which, though it carried with it the germs of degeneracy, yet saved the conventional forms of chamber music from entire stagnation; I mean the *virtuose* element. A number of professed players, travelling virtuosi, made their influence felt from one end of Europe to the other; and it was in consequence of their increased professional skill, of their safe and sure mastery over all technical difficulties, that composers, such as Mozart, animated with a desire to fly at higher game, could venture to give bolder expression to their musical dreams. They could aim at filling the conventional forms with higher and more intensely emotional matter, and could afford to eschew the typical banalities, so dear to the amateurs of their time. At last Beethoven was enabled to clear the entire domains of dilettantism at a bound, and to escape the stifling atmosphere of amateur tastes and wishes. From the beginning of Beethoven's second period, from the Sinfonia Eroica, or the Waldstein Sonata, or the Rasoumouffsky quartettes, we may date the decay and death of dilettantism. These works are absolutely beyond the reach of players who cannot devote an entire life to their instruments, and they form of necessity the centre of that great circle, round which the whole musical world of our day gyrates. With Beethoven, instrumental music rises to a perfectly ideal sphere. He has got rid of all and every conventionality of form and diction. He makes every technical detail subservient to the expression of his poetical idea. He gives himself no trouble as regards difficulties of execution, provided there are no technical impossibilities. He finishes his work as best he can without consulting the desires or tastes of his executants, or even of his audience. The outlines of musical form, such as they have been bequeathed to him by his predecessors, he cannot be said to have either obliterated, or strained. He expanded them, and turned them to account for the expression of intense emotions. It is impossible in all the works of his second and third period to find a trace of conventional and traditional "*shablone*"; every single work forms a perfect organism with its own conditions and laws of being. Every work is a distinct individual, whose connection with the species, may be, and is, traceable, though at first sight it appears to be infinitely removed from the parent stock. I think I can formulize the gist of the whole matter definitely and clearly, when I say, that since Beethoven, it has been the desire of all thinking and aspiring musicians to construct their music upon *a poetical basis*. Now the tendency towards the expression of some definite poetical idea by musical means, is the characteristic of all so-called "Music of the Future," and such a tendency has made itself felt more and more clearly in the works of Beethoven's successors: Mendelssohn, Schumann, Chopin, Berlioz, Liszt, and Wagner. The unlimited powers of the art for emotional expression, and for this only, are being more and more universally recognized; and we are every day moving farther from the old standpoint of vague juggling with sweet sounds in stereotyped forms. A sonata or a string quartette of the old type was built somewhat like an ordinary Victorian house. From the portico and hall of the first theme you were led up a commonplace staircase of runs and passages, to the second theme with its feminine drawing-room air, from whence it was easy to find your way into more or less insignificant accessory rooms, or to descend again into this or that musical pantry.

With Beethoven the form is evolved from the emotional essence of the poetical idea. If you call to mind one of his heavenly Adagios in the string quartettes, or Symphonies, or the astonishing vigour, depth, and tenderness of the opening movements of these works, and compare them to similar movements of Haydn's; or again, if you consider the emotional element in Beethoven's ninth symphony, or in other of the works of his third period, and compare them with any of Mozart's, you will see what I mean to indicate by such an expression as the "Emancipation of form for the sake of the poetical idea," and by the assertion, that such music has a poetical basis, and is the most distinct, and the most perfect of all possible expression of a poetical idea. In Beethoven, the scaffolding of musical forms, conventional and traditional with Haydn and Mozart, in whose works all the props and joists are clearly visible, and often make the impression of empty "*shablone*," is entirely idealized. It is still traceable, but has become entirely subservient to the poetical idea; to the expression of some particular and intense emotion. The structural element, as architects say, has entirely amalgamated with the vital parts. The work of art has ceased to be a conglomerate of tunes, and has become a living organism.

Beethoven has not in every instance thought fit to give a distinct clue to the poetical ideas which prompted, or at least ran parallel with his musical production. He has, however, in his most important works done so, and in the records of his conversations with friends and pupils, many indications in a similar direction are to be found. Thus among his Symphonies, I need only mention the "Eroica," the "Pastoral," and above all the ninth. Among his Sonatas, the "Pathetique," "L'absence et le retour." Among his overtures, "Coriolan," "Egmont," and the three great "Leonoras."

His followers, on the list of German instrumental composers, have carried the principle still farther. Thus to mention an extreme example, and one most likely familiar to you all, Mendelssohn in his overture, entitled "Calm Sea and Prosperous Voyage," follows a poem of Goethe's, line for line, picture for picture. His other concert overtures, "A Midsummer Night's Dream," "Fingal," "Melusine"; all give most vivid impressions of the ideas their titles indicate. Rob. Schumann has in several of his larger works, such as the overtures to "Manfred," "Faust," "Julius Cæsar," succeeded in giving the emotional essence of the poetical works indicated by the titles in the musician's language; and hundreds of his smaller pianoforte pieces have characteristic superscriptions indicative of the precise pictures, images, and feelings that engrossed the composer's mind during his work. In Berlioz's Symphonies, and Liszt's "Poèmes Symphoniques," the new method has reached its climax, and probably in many cases, both composers have overstepped and overstrained the limits and capacities of musical art. Both offer definite, and sometimes very lengthy programmes to their musical work. Berlioz has attempted to make a symphony of Shakspeare's "Romeo and Juliet," and of the fourth Canto of Byron's "Childe Harold." Liszt has reproduced poems by Victor Hugo, Schiller, meditations by Lamartine, scenes from Dante's "Divina Commedia," &c. To my mind, many of these works, marvellously interesting though they be, and surprisingly ingenious and perfect in all technical detail, are a little unpalatable. I get no total and complete impression from them. If I listen to the music, without attending to the poetical programme followed by the composer, the impression is one of musical incoherence; and if I attend to the poem, the conditions of the unsatisfactory impression are only reversed, not done away with. In the one case, I get an amount of very clever music wrapped up in a cloak of poetry; in the other a fine poem stifled under an enormous mass of orchestral colour. What I long for, and what makes Beethoven's works so unapproachable—the entire fusion of the poetical and musical elements into one indissoluble and complete whole—seems wanting. These extreme cases of *programme* music, then, form one phase of the matter, and are known, and sometimes ridiculed, under the sobriquet of "Music of the Future."

The other, the main phase, and the one I entirely agree with, is that represented by Richard Wagner. Wagner, in constructing a musical drama, delineates the characters after the fashion of a great poet, and makes use of music as the ultimate means of poetical expression. Here, as much, and even more than in Liszt's and Berlioz's works, the traditional "*shablones*" of isolated musical form disappear, and the poetical idea everywhere modifies and influences the course and development of the music; but, instead of a separate programme, printed or read, and to be used as a sort of reference, we have the living action on the stage; the emotions, sentiments, and actions, supremely intelligible, developing themselves in actual flesh and blood before our eyes, and the poetical comment to the music, as well as the musical comment to the poetry, uniting into one great focus, and producing a total artistic impression, the intensity of which must by any other conceivable means be for ever unapproachable. In Wagner's dramas, the significance given to music is indicative of the character the art will attain in no very distant future. I firmly believe with him, that music will in time be the matrix, the informing element, of the drama. It was so in ancient Greece, and will be again. I believe that the drama of the future will be as much the richer in scope and power as modern Beethovenian music is richer than Greek music was; and that the complete union which poetry, mimetics, and music have in our time undergone in Wagner's dramas, is a union which will henceforth be inseparable and indissoluble.

It is a common case in all arts and sciences, that a man of genius has some intuitive insight—sees his way by chance as it were through a maze of facts—is perfectly right in his intuition; yet, while seeking plausible reasons for it, such as shall make it palatable to others, hits upon an explanation, only partially true, or mayhap altogether false. His intuitions may for all that be very profound, and perfectly right. Some such intuitive rediscovery of forgotten truth, long lost sight of, we owe to Wagner, and the reasons he has in his voluminous writings given for it, are the cause of all the storms that have devastated the musical world for the last quarter of a century. Wagner is a poet and dramatist by nature and profession; a very different thing from a mere musician, let it be remembered. His artistic talents developed themselves in immediate contact with the musical stage. At starting, he was equipped with a sound classical education over and above his enormous specifically musical and dramatic gifts. Possessing an ardent imagination, that most vividly reproduced the scenes and sentiments of ancient Greek and Elizabethan drama, a fiery soul steeped in the Titanic marvels of Beethoven's symphonies and Bach's fugues, is it to be wondered at that he should have found the dreary everyday routine of operatic banalities, the platitude and puerility of all operatic doings, utterly unbearable? To us, who judge after the fact, it appears most natural and inevitable that he should have attempted a realization of an artistic ideal, made up of these various elements, and intending to combine the separate and isolated perfections of both into one glorious whole; a dramatic whole, in which the poet should be allowed to delineate his characters in accordance with the laws of his art, without making the slightest concession to the musician for the sake of operatic or any other conventionalities; and again, a dramatic whole, in which the musician should be able to give unfettered expansion to the supreme powers for emotional expression, peculiar to his art. Wagner saw the possibilities for such a form clearly before him, and long before he attempted to find plausible reasons in support of his intuition; long before he began to speculate on the subject, he tried to embody his ideal in living works for the musical stage. I need only mention "Der fliegende Holländer," "Tannhäuser," and "Lohengrin," which were all written, and for the most part publicly performed, before a word of his theoretical endeavours saw the light.

He spent the earlier part of his life conducting operas at small theatres in second-rate towns, like Würzburg, Magdeburg, Riga.

During his conductorship at Dresden from 1843 to 1849, he had striven most ardently, but in vain, to raise the general standard of things operatic. After the revolution of '49, finding himself entirely isolated from all musical and dramatic doings, a refugee at Zürich, he determined to give, at least, a written version of the dramatic and musical ideal which he had formed for himself, and which he then saw no hope of actually realizing on the stage. He had received the strongest inducements to account for the stagnation of the musical stage in Germany, by the position it occupies in modern society. It was folly, undoubtedly, to entertain a hope of entirely reversing the position of an institution, which aims almost exclusively at providing diversion and amusement for a population that loves pleasure, because it is bored, and at securing the pecuniary gain necessary to cover the expenses of exhibitions calculated to serve such a purpose. It was folly to think of substituting for this, an ideal tendency to raise the minds of the people from the vulgar interests of daily life, and to enable them to comprehend and to view with due adoration the highest and the most significantly beautiful that the human mind is capable of grasping. Wagner had time to consider the causes which have reduced the stage to the equivocal position it occupies in regard to public life, and on the other hand, to take into account the foundation of those social relations, which would as necessarily produce a theatre such as he conceived it, as modern social relations produce the present theatre. As he had found in the rare appearances of artists of genius a basis for the character of his dramatic-musical ideal, history also presented to him a type for the ideal relation of the theatre, such as he imagined it to public life. This was the theatre of ancient Athens, which was only opened on days of special festivity, when the enjoyment of art was at the same time a religious celebration, and wherein the most distinguished men of the state took part, as poets or actors; appearing like priests before the assembled population of town and country, who were filled with such high expectations of the loftiness of the works to be performed, that Æschylus and Sophocles could produce before them the profoundest of all poems, and be certain of their appreciation. The reasons for the decline of this incomparable work of art were easily perceivable. His attention was first drawn to the social causes of this decline, and they appeared to be identical with the causes of the decline of the ancient state itself. Thence he endeavoured to deduce the social principles of that political organization of the human race, which, correcting the imperfections of the antique state, would be able to found a condition of things, in which the relations between art and public life as it once existed in Athens, would revive in a manner, if possible, more noble, and certainly more durable. His thoughts on this subject were stated in a little pamphlet entitled "Die Kunst und die Revolution" (Art and Revolution). For a comparatively longer time he then occupied himself with inquiring into the character of this deplorable dissolution of the Greek work of art. The first thing that struck him, was the remarkable fact, that those different branches of art which had previously been united in the perfect drama, were now dissolved and separated. The mighty union by which it had been possible to make the most profound and exalted intentions of humanity perfectly intelligible to the people, had ceased to exist, and its component parts—the arts, as we call them—from the position of inspired teachers, had sunk to the level of a pleasant pastime for individual lovers of art, so that while the people were publicly entertained with combats of gladiators and wild beasts, educated men occupied themselves with literature in private seclusion. It was especially important to perceive, that the different arts in their separate and isolated cultivation, however their powers of expression might be increased and developed by brilliant genius, could never (without degenerating into unnaturalness and downright faultiness) aim in any way at replacing that all-powerful work of art, the production of which had only been possible to their combined efforts. With the aid of eminent art critics—Lessing, for instance, in his researches on the limits of painting and poetry—Wagner arrived at the result, that each separate branch of art develops itself to the full extent of its capabilities, and that, arrived at those limits, it cannot overstep them, without incurring the risk of becoming incomprehensible and fantastical, nay, even absurd. At this point, it seemed clear to him, that each art once arrived at its limits, demanded to join itself to a sister art. He seemed, finally, to be able to show it as existing most distinctly and strikingly in the relation between music and poetry; especially taking into consideration the great significance of modern music. Endeavouring thus to imagine a work of art, of which all separate branches are supposed to unite, each in a supreme state of perfection, he arrived spontaneously at a clear conception of that ideal, which his mind had unconsciously formed for itself as a vague vision. As he was unable—remembering the thoroughly faulty relation of the stage to public life—to realize this ideal work of art in our own time, he called it "Kunstwerk der Zukunft" ("Art-work of the future"); and under this title he published a pamphlet, in which he entered into a somewhat detailed exposition of the ideas just mentioned, and it is to this title that we are indebted for that spectral invention—"the Music of the Future," which haunts French and other reports on musical art, and of which you will now easily perceive both the aim and the erroneous origin.

A storm, the like of which had not darkened the musical horizon since Glück carried his operatic reforms in Paris, burst round Wagner's ears directly after the publication of this book. Musical critics, fighting under every manner of flag, and chirping under cover of all manner of variegated feather, made day hideous with their clamour. There was one Herr Bischoff, of Cologne, whose whelping was heard far and wide. It was he who invented "Music of the Future"; that is to say, a sort of music hideous and abominable to our ears, but warranted by its perpetrators to sound all right to the ears of our grandchildren. In a reckless moment Franz Liszt, Wagner's great protagonist, was rash enough to adopt the nickname of "Musician of the Future," much like the "water-guesen" of Holland had adopted their nickname and gloried in it; and ever since that day there has been a strong party of German musicians who have in all humility adopted the sobriquet of "Musicians of the Future."

To sum up. The characteristic feature, then, of what is called "the Music of the Future," as distinguished from the traditional forms of instrumental music, is this: Composers nowadays try to embody a distinct poetical idea, and the musical form is modified and possibly created entirely anew under the influence of this poetical idea; whereas, under the older *régime* of Haydn, Mozart, and their immediate followers, instrumental music was written according to prescribed forms and shapes, in strict accordance with traditional "*shablones*." In Haydn's time the form was paramount, and the poetical element subservient, if it was present at all.

In our day the poetical idea moulds and shapes the form, and music expresses intense emotions, instead of being confined to a sort of arabesque-like combination of pleasant sounds under more or less complicated structural restrictions. The danger of the old method was, that a composer should lay stress upon form, and neglect contents; that he should say things extremely well which were hardly worth saying. With us the danger is, that a composer should express high emotions in a chaotic manner, and that he should attempt to express things in music which the art is incapable of fully realizing.

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## GENERAL MONTHLY MEETING,

Monday, Feb. 3, 1873.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Charles Dent, Esq. B.A.
The Rev. George Smith Drew, M.A.
The Right Hon. Sir James Hannen, Judge of the Probate and Divorce Court.
Richard Budd Painter, M.D.
Henry Sturt, Esq.

were *elected* Members of the Royal Institution.

The Special Thanks of the Members were returned to

SIR CHARLES WHEATSTONE, D.C.L. F.R.S. for his gift of a Magneto-electric Clock with ten dials;

JOHN PETER GASSIOT, Esq. D.C.L. F.R.S. for his gift of a Bust of Mrs. Somerville, by L. Macdonald, Rome, 1844; and to

C. F. HANCOCK, Esq. B.A. for his gift of Littré's "Dictionnaire de la Langue Française." 4 vols. 4to. 1863–72.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

*Actuaries, Institute of*—Journal, No. 89. 8vo. 1872.
*Antiquaries, Society of*—Proceedings, Vol. V. No. 4. 8vo. 1872.
*Astronomical Society, Royal*—Memoirs, Vol. XXXIX. Part 2. 4to. 1872.
Proceedings, Vol. XXXIII. Nos. 1, 2. 8vo. 1872.
*Bavarian Academy of Sciences, Royal*—Verzeichniss von 4093 telescopischen Sterwen. 8vo. 1872.
*British Architects, Royal Institute of*—Sessional Papers, 1872–3. Nos. 2, 3, 4. 4to.

*Carpenter, W. B. LL.D. M.D. F.R.S. (the Author)*—Report on Scientific Researches made during Aug. Sept. and Oct. 1871 in H.M. surveying ship 'Shearwater.' (Royal Soc. Proceedings, XX.) 8vo. 1872.

*Chemical Society*—Journal for Dec. 1872, and Index; and for Jan. 1873. 8vo.

*Church, Professor A. H. M.A.*—Practice with Science: a Series of Agricultural Papers, Vol. I. 8vo. 1867.
Colour. By A. H. Church. 16to. 1873.

*Comitato Geologico d'Italia*—Bollettini. 1872. Nos. 7–10. 8vo. Firenze.

*Director of the Geological Survey*—Mineral Statistics for 1871. 8vo. 1872.
Memoirs. Four Parts. 8vo. 1872.

*Editors*—American Journal of Science for Dec. 1872; and Jan. 1873. 8vo.
Athenæum for Dec. 1872; and Jan. 1873. 4to.
Chemical News for Dec. 1872; and Jan. 1873. 4to.
Engineer for Dec. 1872; and Jan. 1873.
Food Journal for Dec. 1872; and Jan. 1873. 4to.
Journal of Applied Science for Dec. 1872; and Jan. 1873. Folio.
Journal of Gas-Lighting for Dec. 1872; and Jan. 1873. 4to.
Nature for Dec. 1872; and Jan. 1873. 4to.
Nautical Magazine for Dec. 1872; and Jan. 1873. 8vo.
Once a Week for Dec. 1872; and Jan. 1873. 8vo.
Pharmaceutical Journal for Dec. 1872; and Jan. 1873. 8vo.
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## WEEKLY EVENING MEETING,

Friday, Feb. 7, 1873.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

PROFESSOR A. C. RAMSAY, LL.D. F.R.S.

DIRECTOR-GENERAL OF THE GEOLOGICAL SURVEY OF THE UNITED KINGDOM.

### *On Old Continents.*

THE Professor began by stating that in most of the manuals of geology all stratified formations are spoken of as of marine origin, with the exception of part of the Coal-measures, the Purbeck and Wealden beds, and the fresh-water strata of part of the Eocene and Miocene eras.

The object of the discourse was to prove that, in the British Islands, several of the great formations, partly of Palæozoic and partly of Mesozoic or Secondary age, were deposited in large inland lakes, fresh or salt, and if this can be established, then during that epoch there was a long continental epoch in this part of the world analogous to and as important, in a physical point of view, as any of the great continents of the present day.

In general the Upper Silurian seas swarmed with life all marine, but in the very topmost beds these brown strata pass insensibly into the Old Red Sandstone; the marine life rapidly dwindled away, land plants appear in the seeds of Lycopodiaceæ, and the strata finally become red; just such changes as might have taken place if a portion of the sea had been isolated like the Baltic or Black Sea; and in the end being completely separated from the main ocean, and only supplied with rain and the fresh water of rivers, the water became first brackish, and in the end fresh. Referring to the Old Red Sandstone, he said that its red colour was due to the fact that each minute grain of mud or sand was covered by a thin pellicle of peroxide of iron, which could not have been deposited in an open ocean, but might easily be deposited in an inland lake; and that some of the fish of that formation which, except in the very topmost beds, is destitute of shells, have their nearest living analogues in the Polypterus of the rivers of Africa, the Ceratodus of Australia, and in less degree in the Lepidosteus of North America. The topmost beds of the Old Red Sandstone are, however, in places full of ferns and other land plants, and of fresh-water shells of the genus Anodon, just where they gradually pass into the lowest carboniferous strata.

Professor Ramsay then dilated on the great superficial extent and thickness of the carboniferous strata, especially of the Coal-measures; and the evidence which the latter afford of a great continental area which underwent many changes in its physical geography, as shown by the forests and peat mosses of the period, now beds of coal, the existence of sun-cracks and rain-pittings, and footprints of Labyrinthodont amphibia on mud now hardened into shale.

In the red marls and sandstones of the Permian strata, in addition to these signs and the red colour of the rocks, there are sometimes formed pseudomorphous crystals of salt, which crystals could not have been formed in an open ocean, for the water must have been more than saturated with chloride of sodium before the deposition of crystals of salt could have taken place. In the same strata there are footprints and bones of Labyrinthodont amphibia. In regard to the Magnesian Limestone of this period, it was shown that the molluscous life of that epoch was miserably poor in numbers of genera and species when compared with the crowded life of the Carboniferous Limestone epoch; and in this respect it may be compared to the scanty marine fauna of the Caspian Sea at the present day. It was further explained, that as Mollusca do not use magnesia in the formation of their shells, this magnesian limestone could not have been deposited in the ocean, but must have been precipitated in a great inland salt lake, because of evaporation of the water by solar heat. The *Lepidotosaurus Duffii*, which was found in the marly limestone near the base of this formation, was a Labyrinthodont amphibian; and *Proterosaurus Speneri* and *P. Huxleyi*, from the Marl Slate, were probably land Lacertilian reptiles.

It was next shown that the New Red Sandstone and Marl of Britain were deposited in lakes, and the marl certainly in a salt lake. Land plants are frequently found in the sandy beds of that formation; the foraminifera, the bivalve crustacean *Estheria minuta*, and the fish *Dipteronotus cyphus* might just as well have lived in a salt lake as in an open sea; and the small marsupial mammal *Microlestes antiquus*, found in the red marl of Watchet, prove the neighbourhood of land. The Labyrinthodont footprints and other reptilian relics all tend to confirm the view that the strata were deposited in an inland salt lake or lakes, which increased and diminished in size at different seasons of the year; and the gypsum and rock-salt could only have been deposited by solar evaporation of the water of a lake, which, like the Dead Sea, the lake of Utah, or numerous lakes in Central Asia, have no outlets, and only get rid of the water that flows into them by help of the heat of the sun.

This interior portion of Asia, indeed, probably most nearly corresponds to the state of what is now Great Britain and the northern half of Europe during the Permian and New Red epochs.

The result of all the foregoing arguments, in the opinion of the speaker, tends to show that, over much of what is now Europe (and in some other continents) a long continental epoch obtained from the close of the Upper Silurian period down to the beginning of the deposition of the Rhœtic beds, which lie between the New Red Marl and the Lias, a portion of geological time which, in the present state of our knowledge, it is impossible to calculate in years, but which may possibly be comparable to all the time occupied in the deposition of the whole of the Secondary and Tertiary formations.

[A. C. R.]

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## WEEKLY EVENING MEETING,

Friday, February 14, 1873.

William Spottiswoode, Esq. LL.D. M.A. Treasurer R.S. and R.I.
Vice-President, in the Chair.

Robert H. Scott, Esq. F.R.S.
DIRECTOR OF THE METEOROLOGICAL OFFICE,

### *On Recent Progress in Weather Knowledge.*

It is now nearly four years since I had the honour of delivering a lecture in this theatre on the work of the Meteorological Office. On the present occasion my subject is of more general interest, as in treating of recent progress in weather knowledge all over the world we are no longer confined to the operations of our own office.

Although I cannot as yet claim for myself and my brother Meteorologists that we have made serious progress towards what has well been termed, "the splendid possibility of predicting the nature of seasons," I hope still to show you that we have made some advance towards entitling weather knowledge to rank among the sciences, and thereby disproving Arago's famous dictum, that no one who had scientific character to lose would prophesy weather.

The prediction of the seasons, for any considerable period in advance, is of course the problem whose solution must affect the most important social interests, inasmuch as all the operations of agriculture are necessarily dependent on the varying character of the weather. Recently, in order to afford some practical information as to the effect of the weather on growing crops, an agitation has been set on foot for the organization of a system of telegraphic agricultural weather reports, in order by a knowledge of the prospects of the harvest to be able to regulate the price of grain. The veteran Maury* is at the head of this movement, and the question was mooted at the International Statistical Congress at St. Petersburg last summer.

Meanwhile, we ourselves, in the course of last summer, have made a commencement of giving intelligence as to the probable growth of crops, by adding six inland stations to the list of those which furnish information for the Daily Weather Report.

Our recent experience of the rainfall of 1872, which was almost unprecedented and certainly unexpected, both as to its amount and continuance, is a fair illustration of the very moderate pretensions which even the most practised Meteorologists can make to a knowledge of the probable character of the weather for even two months in advance. We are now receiving abundant notes as to the concomitant phenomena of unusual drought during parts of last year in other regions of the earth, and as to the abnormal relations of barometrical pressure over N.E. Europe on the one hand, and Iceland on the other; but none of these facts throw any light, hitherto discoverable, on the causes of our exceptional weather.

Anyone of experience can cite numerous instances of the failure of prophecies of weather based on the popularly received signs, such as the shining of the sun on Candlemas-day, and we shall soon see that the principles on which such prophecies depend are not mathematically correct. It is, nevertheless, undeniable that the movements of birds of passage are apparently directed by a prescience of the coming character of the weather; generally, their arrival may be attributed to the fact that they herald the approach of conditions of weather which have already set in in their home.

It is evident that the changes of weather ought to bear mathematical treatment as well as any other statistical facts, and consequently attempts have been made to apply mathematical reasoning to our experience of the seasons, in order to test whether the popular ideas to which I have just alluded have, or have not, any real basis of truth.

The most recent contribution to our knowledge in this direction is a paper by Wladimir Köppen in the Russian Repertorium für Meteorologie, Vol. II., "On the Sequence of the Non-periodic Variations of Weather, investigated according to the Laws of Probability." The discussion is prefaced by the remark that while weather study has made great progress owing to the development of telegraphy, its results are mainly of utility to the seaman, but remain comparatively valueless for the farmer, while the advantage to be derived from a foreknowledge of the weather is as great in the one case as in the other.

M. Köppen has examined into the chance of a change of weather at any time, and he finds that *the weather has a decided tendency to preserve its character.* Thus, at Brussels, if it has rained for nine or ten days successively, the *next* day will be wet also in four cases out of five: and the chance of a change decreases with the length of time for which the weather *from* which the change is to take place has lasted.

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* The death of Com. Maury on the 1st February has been announced since the lecture was delivered.—R. H. S.

If we take the case of temperature for five-day periods we find the same principle to be true; for if a cold five-day period sets in after warm weather, we can bet two to one that the next such period will be cold too: but if the cold has lasted for two months we can bet nearly eight to one that the first five days of the next month will be cold too. The chance of change is, however, greater for the five-day periods than for single days.

Similar results follow for the months, but here again the chance of change shows an increase.

If we revert to the instance we first cited, that of rain, the result is *not* that if it once begins to rain the chances are in favour of its never ceasing; all that is implied is, that the chances are against its ceasing on a definite day, and that they increase with the length of time the rain has lasted. The problem is similar to that of human life: the chance of a baby one year old living another year is less than that of a man of thirty.

The practical meaning of all this is, that although we know that a compensating anomaly for all extraordinary weather exists somewhere on the earth's surface,—*e. g.* the very common case of intense cold in America while we have a mild winter, which was most strikingly true last January,—there is no reason, as yet ascertained, to anticipate that this compensation will occur at any given place in the course of a year. In other words, when definite conditions of weather have thoroughly established themselves, it is only with great difficulty that the courses of the atmospheric currents are changed.

Attempts have not unfrequently been made to predict the seasons for a long period in advance, but without much success hitherto. One great cause for failure is that accurate meteorological records do not extend beyond the beginning of the century at more than a few stations, and for these we cannot eliminate the local influences altogether. Thus, it is hardly possible to say what has been the approximate temperature of these islands for more than twenty years, —a period far too short for the definite recognition of a cycle. The shortest of these cosmical cycles which has been determined is the sunspot period of 11⅑ years according to Wolf, and there are indications of far longer periods, such as 33 years or even 69⅓ years, according to Hornstein.

At the last meeting of the British Association at Brighton a very interesting paper by Mr. Meldrum, of the Mauritius, was read, in which he showed that the cyclones for which that district of the Indian Ocean enjoys an unenviable notoriety, have been more frequent in some years than in others, and that these years of maximum frequency occurred at intervals of about 11 years, coinciding with those of maximum sunspot frequency.

This agreement is most important, and it has been abundantly corroborated by an examination of the rainfall at such stations in the southern hemisphere as are available. This has been carried out by Mr. Lockyer in 'Nature,' as well as by Mr. Meldrum.

The results, for the comparatively short period to which they refer, are most striking, as they are sufficient to show that a periodicity is traceable in the weather of the Indian Ocean which is eminently suggestive of a close relationship between the changes which take place in the sun's surface, and the phenomena of our own atmosphere.

It will at once be asked, why has not this periodicity, if it exists, been detected long ago by an examination of European records, which are far more complete than any existing for the Indian Ocean? The answer to this is twofold; in the first place, we are pre-eminently in the district of the variable winds, and our storms are not nearly so regular in their character as those of the Mauritius, where almost the sole type of storm is the true tropical cyclone, with its concomitant rainfall. It is next to impossible in this country to keep an accurate record of all the several storms which pass over us. The existence of conjugate storms is not unfrequent; two and even three systems of disturbance being traceable at the same time within the area of the United Kingdom. Are these one single storm or several, and how should they be counted in a catalogue? Rain also cannot be taken as a sign of the frequency of storms in a year; for although we know that warm winters are invariably wet and stormy, it cannot be asserted that the, all but constant, rainfall of the early part of last year was in any way related to storms.

There is, however, in the second place, a far deeper reason for the non-discovery of these cycles in any chance series of rainfall records. The sun passes through phases of greater and less activity, and the terrestrial phenomena corresponding to the epochs of the former character are excessive evaporation in some parts of the globe, and consequent excessive precipitation in others. We must therefore ascertain in what districts we are to look for the one and for the other of these phenomena respectively. The fact of the mutual compensation of anomalies of which I spoke when treating of M. Köppen's paper, will show you that it is not impossible that the years of maximum rainfall at the Mauritius may be years of great dryness here. In fact, we cannot yet say where we shall find the maximum solar effect produced.

The lesson that we are to learn from the fruitful researches of Mr. Meldrum has been pointed out by Mr. Lockyer, that we must aim at attaining a thorough knowledge of the movements and changes of our own atmosphere, and then seek to establish a connection between them and other cosmical phenomena, such as terrestrial magnetism, the relation of which to the state of the sun's surface was pointed out by Sir E. Sabine twenty years ago.

We must now proceed to the subject of Weather Telegraphy and Storm Warnings with which the name of Admiral Fitzroy will always be associated. Justice, however, compels us to admit that this country was not the first to issue telegraphic weather intelligence to its seaports; for in the year 1860, when the possibility of introducing such a system was being discussed here in London, the step had actually been taken in Holland, at the instance of Prof. Buys Ballot. At the present time there is not a single European country, except Greece, which has not its own meteorological organization. In most cases telegraphic weather reports are published in the newspapers, while the example set by Le Verrier, about 1858, of the publication of a lithographed daily bulletin has been followed by our own office, more than 600 copies of whose charts are issued daily to subscribers and for exhibition at sea-ports. Of late years Russia, too, has commenced the publication of a lithographed bulletin.

If we want to see weather telegraphy on its grandest scale, we must cross the Atlantic, where, under the direction of Brigadier-General Myer, no less than three charts are issued every day by the Chief Signal Office of the United States at Washington. This undertaking is rendered possible by the fact that the whole organization is military, and that its efforts are almost entirely concentrated on the preparation of these reports, while the telegraphic system of the States is placed at the disposal of the Signal Office for a certain space of time every day. By this means it is rendered possible to publish the chart and report simultaneously in all the principal cities of the States. It is not unimportant to consider what the system costs, as compared with our own. We spend, at the outside, 4000*l.* a year on our weather telegraphy, when we apportion to it its due share of the expenses of the office, while the vote for the Signal Office, or, to use its familiar designation in the 'New York Herald,' for "Old Probabilities," is no less than 250,000 dollars, about fourteen times as much as our expenditure under the same head.

Our system of stations is shown on the chart, the only station omitted being Christiansund. The first thing noticeable about it is, that we are entirely exposed to the westward, the direction from whence most of our storms come, and that we have little prospect of improving our condition in this respect. In my former lecture I spoke of the proposal to moor vessels off our coasts, having telegraphic communication with the shore, and to use them as floating observatories. The trial that was made with H.M.S. 'Brisk,' at the entrance of the Channel, was anything but a success, as it resulted in a total failure. The proposal has just been resuscitated in the United States by Mr. Morse, who proposes to moor buoys out at sea with observing turrets on them.

Latterly, an announcement has been made that the Portuguese Government intend to establish a reporting system between the Azores and the mainland, and an application has been made to the Meteorological Committee to contribute towards the expense. I need scarcely say that they have at once replied that they would be ready to assist in this particular development of weather telegraphy to the extent of their powers.

We have, in the meantime, instituted a comparison between the 8 A.M. reports from Angra do Heroismo in the Azores and from Valencia for the last 2¾ years, and we find that there is not a single instance in which we can trace the progress of a storm from the Azores to these islands; the two barometrical curves pursue their respective courses almost entirely independently of each other, and so far from its being true "that no country would benefit by this intelligence (from the Azores) so much as Great Britain," as has been asserted, the *primâ facie* evidence afforded by the diagrams is, that the balance of advantage to be gained by the proposed connection would fall to the Azores. It, however, is undeniable that it would be a great advantage to us to know daily what were the atmospheric conditions over the district of the Atlantic where the islands in question are situated.

Our own experience of the value of Transatlantic reports is not satisfactory. For three years we received reports daily (free) from Heart's Content, through the great liberality of the A. A. Telegraph Company, but we did not find them of much practical value; partly because the station was badly placed for wind observations, being in a land-locked bay, and partly because *we can never trust uncorroborated reports from distant stations.* Accordingly, when the committee were asked to pay for the transmission of the reports, they at once decided to discontinue them.

We now come to the practical portion of our subject, the actual *forecasting of the weather*. The only great principle which has been established of late years has been the entire dependence of the wind, as to both its direction and its force, on the barometrical gradient, and not on the absolute height of the barometrical column. This principle is known as Buys Ballot's law, and it is illustrated by the diagram before you. It was fully explained in my former lecture. The law is—"Stand with your back to the wind, and the barometer will be lower on your left hand than on your right." As a simple result of this fertile generalization, we find that there is no danger of a gale, unless the gradient, or difference between barometrical readings over a given distance, exceeds 0·6 in. per 50 miles in the district where the gale will be felt. The attempts which have been made to establish a numerical relation between the gradients and the wind-force have not been satisfactory as yet, owing chiefly to the difficulty of eliminating the local conditions which affect the wind. We are also unable, as yet, to say what interval of time elapses between the establishment of the gradient and the setting in of the corresponding wind, and, on the whole, it must be said that, if we trust to the barometer alone, we shall not have sufficient warning of the approach of a gale.

There are several valuable deductions which follow from Buys Ballot's law.

The wind may move either against or with watch-hands, and the former is the more usual direction of gyration. The former of these motions is termed cyclonic, and takes place round a barometrical minimum; the latter, anti-cyclonic, and takes place round a barometrical maximum. The whole of our weather depends on the relative positions and characters of these areas of defect and excess of atmospherical pressure, and the wind systems connected with them.

A storm may be produced by an increase of pressure as well as by a decrease, and some of our most destructive storms—most destructive, because they give us hardly any warning of their approach—are caused by an increase of pressure on the eastern shores of the North Sea, while a depression is advancing over these islands. The storm of February 6, 1870, which levelled the harbour works at Wick, belonged to this class. Their direction is south-easterly.

Almost all of our storms are related to barometrical depressions, and not to elevations. The reason of this has yet to be explained by the mathematicians. If, therefore, we knew about an advancing depression, the shape (including gradients in all directions), direction, and rate of motion, and whether the disturbance was increasing or diminishing in intensity, we should be able to form a fair judgment as to what parts of the coast would be most likely to feel a gale, and from what points of the compass.

There is hardly one of these particulars of which we can gain a sufficient knowledge until the storm is well upon us, so that the issue of warnings to our exposed western and northern coasts will ever be a matter of great difficulty and uncertainty.

As regards the direction of motion of storms, we have some progress in knowledge to report. Professor Mohn, in his Storm Atlas, has assigned for a few storms the direction and rate of advance, and has shown how both these elements are modified as the storm moves across Scandinavia into Russia. An English meteorologist, Mr. Ley, has also paid attention to the subject; and in his recent work, 'The Laws of the Winds in Western Europe,' has given charts of the mean paths of depressions for certain months of the year.

Both of these investigators refer the direction of motion to the distribution and condensation of vapour in the atmosphere; and Mr. Ley maintains that the depression itself is generated by excessive rainfall, and that its advance is due to the same cause. We are hardly prepared to admit the truth of this statement, in its entirety; the area covered by our daily weather reports is too small for us to be able to test the matter thoroughly, and but few of the foreign stations give either vapour tension or rainfall. Moreover, the magnitude of the depressions affords an argument against their being simply due to the condensation of vapour; for on November 22, 1869, barometrical readings were reduced to the extent of nearly an inch, from what they had been on the 21st, over an area of about 200,000 square miles. To take a recent instance, on the 20th of January, the deficit of atmospherical pressure amounted to about $\frac{1}{20}$th of its total amount over the United Kingdom, the readings ranging between 28·0 and 28·5, instead of between 29·5 and 30·0.

We know that the direction of motion is ruled very much by the position of the areas of high pressure, which are of considerable superficial extent, and, as a rule, are not subject to much motion of translation. The depressions appear to skirt round these areas of high pressure, and not to advance into them. There is, unfortunately, not time for me to point out to you the results to which we have been led by our study of the mutual relations of these areas of high and low pressure; but as an illustration of the effects of an area of high pressure on our weather, I may say that one of the worst signs for us of the danger of a south-westerly gale is to find the barometer over the south of France high and rising.

The simplest idea I can give you of the general motion of these areas of depression is, that they follow each other in the main stream of air, which sweeps round the permanent area of low pressure near Iceland, much in the same way as eddies in a running stream. I have prepared two photographs which, by the kindness of the authorities here, I am enabled to throw on the screen, and of which the first, for Feb. 16, 1872, shows the co-existence of two areas of high and two of low pressure, while the second, for Jan. 24, 1872, shows two distinct storms, the centre of one being at Scarborough, and of the other off Valencia.

Many of the depressions appear to be modified in their character by the contour of the land. We have more than once had an instance of a storm which apparently struck the land about Valencia, travelled northwards until it found an opening in the coast-line, such as Donegal Bay, and then crossed to the Irish Sea, or else skirted round Ireland and crossed the Lowlands of Scotland.

In some cases which we are as yet quite unable to anticipate, the disturbance changes the direction of its motion entirely, and returns for a time on its former path. This was in a marked way the case with the storm of April 21, 1872, which came down over Ireland, between Valencia and Armagh, swept round along the north coast of France up to Havre, recoiled and passed north of Portland and Falmouth to the south of Ireland, and eventually travelled out to sea across Ireland, nearly in the direction along which it had arrived.

Such a storm as this sets us quite astray, and makes our warnings quite wrong. Another circumstance which complicates the study of storms most seriously, is that the depressions increase or decrease in their intensity, and of the rate of this change we are quite ignorant.

I have already alluded to storms which are mainly due to a rise of pressure, and I have selected one of these in February, 1871, for an illustration. This depression advanced from the westward, but hardly assumed the character of a storm until the morning of the 10th of February, when its centre lay over the north of England. Now, between the 9th and 10th the barometer over the North Sea had risen briskly, so that when the new depression came it found the conditions favourable to high gradients and south-east gales on its eastern side. Its appearance was quite unexpected by us; for even at 6 P.M. on the 9th there were little signs of it, and, moreover, its force at 8 A.M. on the 10th was moderate compared with its fury later on in the day. The centre finally passed over London, and the whole phenomenon passed on to the Continent, where it is untraceable, in consequence of the absence of reports, owing to the war.

Easterly gales are, on the whole, difficult for us to foresee; they come on us with little warning; and this is not solely attributable to the deficiency of information from Eastern Europe, for since we have received warnings from that region, we have hardly had an instance of a warning which preceded the gale. In most instances the easterly storm was on the north side of a depression travelling eastwards, and began first at our western stations. Such was the storm of Sunday, Feb. 2, which set in as an east gale at Pembroke, and subsequently extended to stations lying to the eastward. Some of these easterly storms do apparently advance from the eastward, and to this class belongs the fearful storm of Nov. 12–13, in the Baltic. The ravages of this storm were mainly caused by the fact, that a continuance of westerly winds had dammed back the water at the Skager Rack, and then, when the wind chopped round to east, the narrow channels between the Danish Islands could not discharge the water quickly enough. The unavoidable result was an inundation.

What are now the signs of a storm, and when do we issue warnings? We are perforce driven to use the barometer mainly, as it is an instrument more closely related to the direction and force of the wind than the thermometer, and one whose daily range is trifling; as we have seen already, if we trust it alone, we shall hardly ever be certain about a storm, and the thermometer will not help us much. We have, then, other signs, such as shifts in the direction of the wind, an increase of sea, and all the manifold local indications in the atmosphere given by the character of the clouds and the transparency, &c., of the air, which are invaluable as collateral information, but require a practised eye to discern them.

If it were possible to place our office, with its present telegraphic facilities, on the west coast of Ireland, we might fairly hope to foretell five-sixths of the storms which strike us. On two separate occasions I have been in the district in question, and knew perfectly well from the look of the sky that a storm was coming some hours before the barometer began to fall, and consequently long before the office here could issue warnings.

Some storms do not give even as much warning as what I have indicated. That of Feb. 10, 1871, is one instance, and that of Nov. 22, 1872, when the 'Royal Adelaide' was lost on Chesil Bank, is a recent case. On that day at noon the telegraphic reports showed an apparent improvement in the weather on that of the previous day, so that I lowered the drums on our south coast. At night the gale came, and of course the comments on the office were not favourable. Fortunately for our own peace of mind, one of our best sea observers, Capt. Thos. Donkin, of the 'Inverness,' which was one of the three ships that rode out the Madras cyclone of May 2, 1872, was out in that gale and was blown back by it, hove-to, from the Lizard to the Casquets. I wrote to him to Portland to ask him whether he had anticipated the storm from the look of the sky, and his answer was:

"With respect to the weather on the 22nd November, I may say that at noon I was standing in towards the land, between Falmouth and Plymouth, and a pilot cutter came alongside, and if I had had the least apprehension of such a gale as followed being near at hand, I should have taken a pilot and gone into Plymouth. The appearance of the weather at the time was fine, though the glass was falling, though not low at the time for S.W. wind and unsettled weather."

This will, I hope, show my audience that we cannot foretell all storms by means of telegraphy. We are beginning to recognize other signs of disturbance in the atmosphere generally as indications of a storm, but these can hardly as yet be considered as scientifically exact. Among these we may mention the circumstance which Prof. Mohn has noticed, that warning of south-westerly gales for the coast of Norway is given by a rise of temperature at Dovre at a height of 2100 feet above the sea. In these islands we have little prospect of availing ourselves of this source of information, as our telegraphic stations are all at low levels.

Another principle, which was fully explained in my former lecture, is that pointed out by Mr. Meldrum, that storms are generated between two currents of air flowing in opposite directions, the easterly winds being on the polar side of the westerly. Two instances of this are shown on the diagrams, but from them—especially in July, 1867—it does not appear that the East wind was thoroughly established as a current, and it seems more probable that the light E. airs were only due to an indraught towards the area of depression, just as, before many of our gales, we see the wind on the west coast of France drawing into S.E., while the direction at Valencia is S.W.

In conclusion, I must say a few words as to the degree to which we are practically able to foretell storms, and of the results of our storm-warning messages. It may seem a comparatively simple matter to say whether or not a warning was issued in time or not; but it is really not so easy a matter, as storms are exceedingly local, so that two observers situated close to each other may differ seriously as to whether there has been a gale or not. We have endeavoured, by the best means in our power, to keep a check on the correctness of our warnings for the last three years; that is, on whether or not we have been able to give intelligence of the gales in time to be of practical service to seamen. The result has been for the years 1870-71, that 46 per cent. of our warnings have been followed by gales, and about 20 per cent. in addition have been justified by the occurrence of strong winds after they were hoisted, showing a total percentage of successful warnings of nearly 70 per cent. For the first six months of 1872 the percentage of gales for which warnings were issued in time has risen above 60, and the total percentage of successes to nearly 80, owing to the two independent circumstances that the postal telegraphic arrangements have gradually become more perfect, and that the exchange of information with the Continent has become more regular since peace has been re-established.

[R. H. S.]

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## WEEKLY EVENING MEETING,

Friday, February 21, 1873.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

Professor J. Clerk Maxwell, M.A. F.R.S.

### *On Action at a Distance.*

I have no new discovery to bring before you this evening. I must ask you to go over very old ground, and to turn your attention to a question which has been raised again and again ever since men began to think.

The question is that of the transmission of force. We see that two bodies at a distance from each other exert a mutual influence on each other's motion. Does this mutual action depend on the existence of some third thing, some medium of communication, occupying the space between the bodies, or do the bodies act on each other immediately, without the intervention of anything else?

The mode in which Faraday was accustomed to look at phenomena of this kind differs from that adopted by many other modern inquirers, and my special aim will be to enable you to place yourselves at Faraday's point of view, and to point out the scientific value of that conception of *lines of force* which, in his hands, became the key to the science of electricity.

When we observe one body acting on another at a distance, before we assume that this action is direct and immediate, we generally inquire whether there is any material connection between the two bodies; and if we find strings, or rods, or mechanism of any kind, capable of accounting for the observed action between the bodies, we prefer to explain the action by means of these intermediate connections, rather than to admit the notion of direct action at a distance.

Thus, when we ring a bell by means of a wire, the successive parts of the wire are first tightened and then moved, till at last the bell is rung at a distance by a process in which all the intermediate particles of the wire have taken part one after the other. We may ring a bell at a distance in other ways, as by forcing air into a long tube, at the other end of which is a cylinder with a piston which is made to fly out and strike the bell. We may also use a wire; but instead of pulling it, we may connect it at one end with a voltaic battery, and at the other with an electro-magnet, and thus ring the bell by electricity.

Here are three different ways of ringing a bell. They all agree, however, in the circumstance that between the ringer and the bell there is an unbroken line of communication, and that at every point of this line some physical process goes on by which the action is transmitted from one end to the other. The process of transmission is not instantaneous, but gradual; so that there is an interval of time after the impulse has been given to one extremity of the line of communication, during which the impulse is on its way, but has not reached the other end.

It is clear, therefore, that in many cases the action between bodies at a distance may be accounted for by a series of actions between each successive pair of a series of bodies which occupy the intermediate space; and it is asked, by the advocates of mediate action, whether, in those cases in which we cannot perceive the intermediate agency, it is not more philosophical to admit the existence of a medium which we cannot at present perceive, than to assert that a body can act at a place where it is not.

To a person ignorant of the properties of air, the transmission of force by means of that invisible medium would appear as unaccountable as any other example of action at a distance, and yet in this case we can explain the whole process, and determine the rate at which the action is passed on from one portion to another of the medium.

Why then should we not admit that the familiar mode of communicating motion by pushing and pulling with our hands is the type and exemplification of all action between bodies, even in cases in which we can observe nothing between the bodies which appears to take part in the action?

Here for instance is a kind of attraction with which Professor Guthrie has made us familiar. A disk is set in vibration, and is then brought near a light suspended body, which immediately begins to move towards the disk, as if drawn towards it by an invisible cord. What is this cord? Sir W. Thomson has pointed out that in a moving fluid the pressure is least where the velocity is greatest. The velocity of the vibratory motion of the air is greatest nearest the disk. Hence the pressure of the air on the suspended body is less on the side nearest the disk than on the opposite side, the body yields to the greater pressure, and moves toward the disk.

The disk, therefore, does not act where it is not. It sets the air next it in motion by pushing it, this motion is communicated to more and more distant portions of the air in turn, and thus the pressures on opposite sides of the suspended body are rendered unequal, and it moves towards the disk in consequence of the excess of pressure. The force is therefore a force of the old school—a case of *vis a tergo*—a shove from behind.

The advocates of the doctrine of action at a distance, however, have not been put to silence by such arguments. What right, say they, have we to assert that a body cannot act where it is not? Do we not see an instance of action at a distance in the case of a magnet, which acts on another magnet not only at a distance, but with the most complete indifference to the nature of the matter which occupies the intervening space? If the action depends on something occupying the space between the two magnets, it cannot surely be a matter of indifference whether this space is filled with air or not, or whether wood, glass, or copper, be placed between the magnets.

Besides this, Newton's law of gravitation, which every astronomical observation only tends to establish more firmly, asserts not only that the heavenly bodies act on one another across immense intervals of space, but that two portions of matter, the one buried a thousand miles deep in the interior of the earth, and the other a hundred thousand miles deep in the body of the sun, act on one another with precisely the same force as if the strata beneath which each is buried had been non-existent. If any medium takes part in transmitting this action, it must surely make some difference whether the space between the bodies contains nothing but this medium, or whether it is occupied by the dense matter of the earth or of the sun.

But the advocates of direct action at a distance are not content with instances of this kind, in which the phenomena, even at first sight, appear to favour their doctrine. They push their operations into the enemy's camp, and maintain that even when the action is apparently the pressure of contiguous portions of matter, the contiguity is only apparent—that a space *always* intervenes between the bodies which act on each other. They assert, in short, that so far from action at a distance being impossible, it is the only kind of action which ever occurs, and that the favourite old *vis a tergo* of the schools has no existence in nature, and exists only in the imagination of schoolmen.

The best way to prove that when one body pushes another it does not touch it, is to measure the distance between them. Here are two glass lenses, one of which is pressed against the other by means of a weight. By means of the electric light we may obtain on the screen an image of the place where the one lens presses against the other. A series of coloured rings is formed on the screen. These rings were first observed and first explained by Newton. The particular colour of any ring depends on the distance between the surfaces of the pieces of glass. Newton formed a table of the colours corresponding to different distances, so that by comparing the colour of any ring with Newton's table, we may ascertain the distance between the surfaces at that ring. The colours are arranged in rings because the surfaces are spherical, and therefore the interval between the surfaces depends on the distance from the line joining the centres of the spheres. The central spot of the rings indicates the place where the lenses are nearest together, and each successive ring corresponds to an increase of about the 4000th part of a millimètre in the distance of the surfaces.

The lenses are now pressed together with a force equal to the weight of an ounce; but there is still a measurable interval between them, even at the place where they are nearest together. They are not in optical contact. To prove this, I apply a greater weight. A new colour appears at the central spot, and the diameters of all the rings increase. This shows that the surfaces are now nearer than at first, but they are not yet in optical contact, for if they were, the central spot would be black. I therefore increase the weights, so as to press the lenses into optical contact.

But what we call optical contact is not real contact. Optical contact indicates only that the distance between the surfaces is much less than a wave-length of light. To show that the surfaces are not in real contact, I remove the weights. The rings contract, and several of them vanish at the centre. Now it is possible to bring two pieces of glass so close together, that they will not tend to separate at all, but adhere together so firmly, that when torn asunder the glass will break, not at the surface of contact, but at some other place. The glasses must then be many degrees nearer than when in mere optical contact.

Thus we have shown that bodies begin to press against each other whilst still at a measurable distance, and that even when pressed together with great force they are not in absolute contact, but may be brought nearer still, and that by many degrees.

Why, then, say the advocates of direct action, should we continue to maintain the doctrine, founded only on the rough experience of a pre-scientific age, that matter cannot act where it is not, instead of admitting that all the facts from which our ancestors concluded that contact is essential to action were in reality cases of action at a distance, the distance being too small to be measured by their imperfect means of observation?

If we are ever to discover the laws of nature, we must do so by obtaining the most accurate acquaintance with the facts of nature, and not by dressing up in philosophical language the loose opinions of men who had no knowledge of the facts which throw most light on these laws. And as for those who introduce ætherial, or other media, to account for these actions, without any direct evidence of the existence of such media, or any clear understanding of how the media do their work, and who fill all space three and four times over with æthers of different sorts, why the less these men talk about their philosophical scruples about admitting action at a distance the better.

If the progress of science were regulated by Newton's first law of motion, it would be easy to cultivate opinions in advance of the age. We should only have to compare the science of to-day with that of fifty years ago; and by producing, in the geometrical sense, the line of progress, we should obtain the science of fifty years hence.

The progress of science in Newton's time consisted in getting rid of the celestial machinery with which generations of astronomers had encumbered the heavens, and thus "sweeping cobwebs off the sky."

Though the planets had already got rid of their crystal spheres, they were still swimming in the vortices of Descartes. Magnets were surrounded by effluvia, and electrified bodies by atmospheres, the properties of which resembled in no respect those of ordinary effluvia and atmospheres.

When Newton demonstrated that the force which acts on each of the heavenly bodies depends on its relative position with respect to the other bodies, the new theory met with violent opposition from the advanced philosophers of the day, who described the doctrine of gravitation as a return to the exploded method of explaining everything by occult causes, attractive virtues, and the like.

Newton himself, with that wise moderation which is characteristic of all his speculations, answered that he made no pretence of explaining the mechanism by which the heavenly bodies act on each other. To determine the mode in which their mutual action depends on their relative position was a great step in science, and this step Newton asserted that he had made. To explain the process by which this action is effected was a quite distinct step, and this step Newton, in his 'Principia,' does not attempt to make.

But so far was Newton from asserting that bodies really do act on one another at a distance, independently of anything between them, that in a letter to Bentley, which has been quoted by Faraday in this place, he says:—

"It is inconceivable that inanimate brute matter should, without the mediation of something else, which is not material, operate upon and affect other matter without mutual contact, as it must do if gravitation, in the sense of Epicurus, be essential and inherent in it. . . . That gravity should be innate, inherent, and essential to matter, so that one body can act upon another at a distance, through a vacuum, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it."

Accordingly, we find in his 'Optical Queries,' and in his letters to Boyle, that Newton had very early made the attempt to account for gravitation by means of the pressure of a medium, and that the reason he did not publish these investigations "proceeded from hence only, that he found he was not able, from experiment and observation, to give a satisfactory account of this medium, and the manner of its operation in producing the chief phenomena of nature."*

The doctrine of direct action at a distance cannot claim for its author the discoverer of universal gravitation. It was first asserted by Roger Cotes, in his preface to the 'Principia,' which he edited during Newton's life. According to Cotes, it is by experience that we learn that all bodies gravitate. We do not learn in any other way that they are extended, movable, or solid. Gravitation, therefore, has as much right to be considered an essential property of matter as extension, mobility, or impenetrability.

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* Maclaurin's Account of Newton's Discoveries.

And when the Newtonian philosophy gained ground in Europe, it was the opinion of Cotes rather than that of Newton that became most prevalent, till at last Boscovich propounded his theory, that matter is a congeries of mathematical points, each endowed with the power of attracting or repelling the others according to fixed laws. In his world, matter is unextended, and contact is impossible. He did not forget, however, to endow his mathematical points with inertia. In this some of the modern representatives of his school have thought that he "had not quite got so far as the strict modern view of 'matter' as being but an expression for modes or manifestations of 'force.'" *

But if we leave out of account for the present the development of the ideas of science, and confine our attention to the extension of its boundaries, we shall see that it was most essential that Newton's method should be extended to every branch of science to which it was applicable—that we should investigate the forces with which bodies act on each other in the first place, before attempting to explain *how* that force is transmitted. No men could be better fitted to apply themselves exclusively to the first part of the problem, than those who considered the second part quite unnecessary.

Accordingly Cavendish, Coulomb, and Poisson, the founders of the exact sciences of electricity and magnetism, paid no regard to those old notions of "magnetic effluvia" and "electric atmospheres," which had been put forth in the previous century, but turned their undivided attention to the determination of the law of force, according to which electrified and magnetized bodies attract or repel each other. In this way the true laws of these actions were discovered, and this was done by men who never doubted that the action took place at a distance, without the intervention of any medium, and who would have regarded the discovery of such a medium as complicating rather than as explaining the undoubted phenomena of attraction.

We have now arrived at the great discovery by Örsted of the connection between electricity and magnetism. Örsted found that an electric current acts on a magnetic pole, but that it neither attracts it nor repels it, but causes it to move round the current. He expressed this by saying that "the electric conflict acts in a revolving manner."

The most obvious deduction from this new fact was that the action of the current on the magnet is not a push-and-pull force, but a rotatory force, and accordingly many minds were set a-speculating on vortices and streams of æther whirling round the current.

But Ampère, by a combination of mathematical skill with experimental ingenuity, first proved that two electric currents act on one another, and then analysed this action into the resultant of a system of push-and-pull forces between the elementary parts of these currents.

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* Review of Mrs. Somerville, 'Saturday Review,' Feb. 13, 1869.

The formula of Ampère, however, is of extreme complexity, as compared with Newton's law of gravitation, and many attempts have been made to resolve it into something of greater apparent simplicity.

I have no wish to lead you into a discussion of any of these attempts to improve a mathematical formula. Let us turn to the independent method of investigation employed by Faraday in those researches in electricity and magnetism which have made this Institution one of the most venerable shrines of science.

No man ever more conscientiously and systematically laboured to improve all his powers of mind than did Faraday from the very beginning of his scientific career. But whereas the general course of scientific method then consisted in the application of the ideas of mathematics and astronomy to each new investigation in turn, Faraday seems to have had no opportunity of acquiring a technical knowledge of mathematics, and his knowledge of astronomy was mainly derived from books.

Hence, though he had a profound respect for the great discovery of Newton, he regarded the attraction of gravitation as a sort of sacred mystery, which, as he was not an astronomer, he had no right to gainsay or to doubt, his duty being to believe it in the exact form in which it was delivered to him. Such a dead faith was not likely to lead him to explain new phenomena by means of direct attractions.

Besides this, the treatises of Poisson and Ampère are of so technical a form, that to derive any assistance from them the student must have been thoroughly trained in mathematics, and it is very doubtful if such a training can be begun with advantage in mature years.

Thus Faraday, with his penetrating intellect, his devotion to science, and his opportunities for experiments, was debarred from following the course of thought which had led to the achievements of the French philosophers, and was obliged to explain the phenomena to himself by means of a symbolism which he could understand, instead of adopting what had hitherto been the only tongue of the learned.

This new symbolism consisted of those lines of force extending themselves in every direction from electrified and magnetic bodies, which Faraday in his mind's eye saw as distinctly as the solid bodies from which they emanated.

The idea of lines of force and their exhibition by means of iron filings was nothing new. They had been observed repeatedly, and investigated mathematically as an interesting curiosity of science. But let us hear Faraday himself, as he introduces to his reader the method which in his hands became so powerful.*

"It would be a voluntary and unnecessary abandonment of most valuable aid if an experimentalist, who chooses to consider magnetic power as represented by lines of magnetic force, were to deny himself the use of iron filings. By their employment he may make many conditions of the power, even in complicated cases, visible to the eye at once, may trace the varying direction of the lines of force and determine the relative polarity, may observe in which direction the power is increasing or diminishing, and in complex systems may determine the neutral points, or places where there is neither polarity nor power, even when they occur in the midst of powerful magnets. By their use probable results may be seen at once, and many a valuable suggestion gained for future leading experiments."

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* Exp. Res. 3284.

## *Experiment on Lines of Force.*

In this experiment each filing becomes a little magnet. The poles of opposite names belonging to different filings attract each other and stick together, and more filings attach themselves to the exposed poles, that is, to the ends of the row of filings. In this way the filings, instead of forming a confused system of dots over the paper, draw together, filing to filing, till long fibres of filings are formed, which indicate by their direction the lines of force in every part of the field.

The mathematicians saw in this experiment nothing but a method of exhibiting at one view the direction in different places of the resultant of two forces, one directed to each pole of the magnet; a somewhat complicated result of the simple law of force.

But Faraday, by a series of steps as remarkable for their geometrical definiteness as for their speculative ingenuity, imparted to his conception of these lines of force a clearness and precision far in advance of that with which the mathematicians could then invest their own formulæ.

In the first place, Faraday's lines of force are not to be considered merely as individuals, but as forming a system, drawn in space in a definite manner, so that the number of the lines which pass through an area, say of one square inch, indicates the intensity of the force acting through the area. Thus the lines of force become definite in number. The strength of a magnetic pole is measured by the number of lines which proceed from it; the electro-tonic state of a circuit is measured by the number of lines which pass through it.

In the second place, each individual line has a continuous existence in space and time. When a piece of steel becomes a magnet or when an electric current begins to flow, the lines of force do not start into existence each in its own place, but as the strength increases new lines are developed within the magnet or current, and gradually grow outwards, so that the whole system expands from within, like Newton's rings in our former experiment. Thus every line of force preserves its identity during the whole course of its existence, though its shape and size may be altered to any extent.

I have no time to describe the methods by which every question relating to the forces acting on magnets or on currents, or to the induction of currents in conducting circuits, may be solved by the consideration of Faraday's lines of force. In this place they can never be forgotten. By means of this new symbolism, Faraday defined with mathematical precision the whole theory of electro-magnetism, in language free from mathematical technicalities, and applicable to the most complicated as well as the simplest cases. But Faraday did not stop here. He went on from the conception of geömetrical lines of force to that of physical lines of force. He observed that the motion which the magnetic or electric force tends to produce is invariably such as to shorten the lines of force and to allow them to spread out laterally from each other. He thus perceived in the medium a state of stress, consisting of a tension, like that of a rope, in the direction of the lines of force, combined with a pressure in all directions at right angles to them.

This is quite a new conception of action at a distance, reducing it to a phenomenon of the same kind as that action at a distance which is exerted by means of the tension of ropes and the pressure of rods. When the muscles of our bodies are excited by that stimulus which we are able in some unknown way to apply to them, the fibres tend to shorten themselves and at the same time to expand laterally. A state of stress is produced in the muscle, and the limb moves. This explanation of muscular action is by no means complete. It gives no account of the cause of the excitement of the state of stress, nor does it even investigate those forces of cohesion which enable the muscles to support this stress. Nevertheless, the simple fact, that it substitutes a kind of action which extends continuously along a material substance for one of which we know only a cause and an effect at a distance from each other, induces us to accept it as a real addition to our knowledge of animal mechanics.

For similar reasons we may regard Faraday's conception of a state of stress in the electro-magnetic field as a method of explaining action at a distance by means of the continuous transmission of force, even though we do not know how the state of stress is produced.

But one of Faraday's most pregnant discoveries, that of the magnetic rotation of polarized light, enables us to proceed a step farther. The phenomenon, when analysed into its simplest elements, may be described thus:—Of two circularly polarized rays of light, precisely similar in configuration, but rotating in opposite directions, that ray is propagated with the greater velocity which rotates in the same direction as the electricity of the magnetizing current.

It follows from this, as Sir W. Thomson has shown by strict dynamical reasoning, that the medium when under the action of magnetic force must be in a state of rotation—that is to say, that small portions of the medium, which we may call molecular vortices, are rotating, each on its own axis, the direction of this axis being that of the magnetic force.

Here, then, we have an explanation of the tendency of the lines of magnetic force to spread out laterally and to shorten themselves. It arises from the centrifugal force of the molecular vortices.

The mode in which electromotive force acts in starting and stopping the vortices is more abstruse, though it is of course consistent with dynamical principles.

We have thus found that there are several different kinds of work to be done by the electro-magnetic medium if it exists. We have also seen that magnetism has an intimate relation to light, and we know that there is a theory of light which supposes it to consist of the vibrations of a medium. How is this luminiferous medium related to our electro-magnetic medium?

It fortunately happens that electro-magnetic measurements have been made from which we can calculate by dynamical principles the velocity of propagation of small magnetic disturbances in the supposed electro-magnetic medium.

This velocity is very great, from 288 to 314 millions of mètres per second, according to different experiments. Now the velocity of light, according to Foucault's experiments, is 298 millions of mètres per second. In fact, the different determinations of either velocity differ from each other more than the estimated velocity of light does from the estimated velocity of propagation of small electro-magnetic disturbance. But if the luminiferous and the electro-magnetic media occupy the same place, and transmit disturbances with the same velocity, what reason have we to distinguish the one from the other? By considering them as the same, we avoid at least the reproach of filling space twice over with different kinds of æther.

Besides this, the only kind of electro-magnetic disturbances which can be propagated through a non-conducting medium is a disturbance transverse to the direction of propagation, agreeing in this respect with what we know of that disturbance which we call light. Hence, for all we know, light also may be an electro-magnetic disturbance in a non-conducting medium. If we admit this, the electro-magnetic theory of light will agree in every respect with the undulatory theory, and the work of Thomas Young and Fresnel will be established on a firmer basis than ever, when joined with that of Cavendish and Coulomb by the keystone of the combined sciences of light and electricity—Faraday's great discovery of the electro-magnetic rotation of light.

The vast interplanetary and interstellar regions will no longer be regarded as waste places in the universe, which the Creator has not seen fit to fill with the symbols of the manifold order of His kingdom. We shall find them to be already full of this wonderful medium; so full, that no human power can remove it from the smallest portion of space, or produce the slightest flaw in its infinite continuity. It extends unbroken from star to star; and when a molecule of hydrogen vibrates in the dogstar, the medium receives the impulses of these vibrations; and after carrying them in its immense bosom for three years, delivers them in due course, regular order, and full tale into the spectroscope of Mr. Huggins, at Tulse Hill.

But the medium has other functions and operations besides bearing light from man to man, and from world to world, and giving evidence of the absolute unity of the metric system of the universe. Its minute parts may have rotatory as well as vibratory motions, and the axes of rotation form those lines of magnetic force which extend in unbroken continuity into regions which no eye has seen, and which, by their action on our magnets, are telling us in language not yet interpreted, what is going on in the hidden underworld from minute to minute and from century to century.

And these lines must not be regarded as mere mathematical abstractions. They are the directions in which the medium is exerting a tension like that of a rope, or rather, like that of our own muscles. The tension of the medium in the direction of the earth's magnetic force is in this country one grain weight on eight square feet. In some of Dr. Joule's experiments, the medium has exerted a tension of 200 lbs. weight per square inch.

But the medium, in virtue of the very same elasticity by which it is able to transmit the undulations of light, is also able to act as a spring. When properly wound up, it exerts a tension, different from the magnetic tension, by which it draws oppositely electrified bodies together, produces effects through the length of telegraph wires, and when of sufficient intensity, leads to the rupture and explosion called lightning.

These are some of the already discovered properties of that which has often been called vacuum, or nothing at all. They enable us to resolve several kinds of action at a distance into actions between contiguous parts of a continuous substance. Whether this resolution is of the nature of explication or complication, I must leave to the metaphysicians.

[J. C. M.]

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## WEEKLY EVENING MEETING,

Monday, February 28, 1873.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

General Sir Henry C. Rawlinson, K.C.B. D.C.L. F.R.S. Pres. Roy. Geog. Soc.

### *On Livingstone's Explorations in Africa.*

[Abstract deferred.]

## GENERAL MONTHLY MEETING,

### Monday, March 3, 1873.

Sir Henry Holland, Bart. M.D. D.C.L. President, in the Chair.

Joseph Bedder, Esq.
J. S. Bergheim, Esq.
Matthew Blakiston, Esq.
John Brogden, Esq.
The Rev. Robert Henry Nisbett Browne, M.A.
George Edward Chapman, Esq.
The Lady Mary F. Egerton.
Stephen Henry Emmens, Esq.
Lewis A. Jackson, Esq.
Charles P. Matthews, Esq.
John W. Miers, Esq.
Frank Normandy, Esq.
Professor William Odling, M.A. F.R.S.
William Miller Ord, M.D.
Thomas Levett Prinsep, Esq. J.P.
Arthur Rigg, jun. Esq.
Mrs. C. William Siemens.
Mrs. Otto Siemens.
Reginald Masters Smith, Esq.
Lionel A. Walford, Esq.
J. P. Ward, Esq.

were *elected* Members of the Royal Institution.

Professor Tyndall having presented to the Royal Institution the new Philosophical Apparatus employed by him in his Lectures recently delivered in the United States, it was moved by Sir Frederick Pollock, Bart., seconded by W. Pole, Esq. F.R.S., and unanimously

Resolved, That the warmest congratulations of the Members of the Royal Institution be offered to their Professor of Natural Philosophy upon his arrival in England from the United States of America, in which, upon the invitation of the most eminent scientific men of America, he has been recently delivering a series of Lectures unexampled for the interest they have created in that country, and the large and distinguished audiences who have been attracted to them.

The Members of the Royal Institution rejoice that the people of America have shared in the advantages of Professor Tyndall's teaching and illustrations of those sciences which have been so greatly advanced by the labours of his predecessors, and by his own, in the laboratories of the Royal Institution.

They receive and welcome him, on his return to what they are proud to be able to designate as his own scientific home, with satisfaction and delight, and wish him all continued health and prosperity.

The Members of the Royal Institution have also to thank Professor Tyndall for his liberal gift to the Institution of the splendid and extensive Apparatus employed by him in his Lectures in America, and congratulate him on the generous spirit, and the love of science, which has led him to appropriate the profits of his Lectures in the United States to the establishment of a fund to assist the scientific studies of young Americans in Europe.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.: —

FROM

*Alcock, Col. Z. St. L. M.R.I. (the Author)*—Relative Power of Nations. Third Edition. (K G S) 8vo. 1873.
*Astronomical Society, Royal*—Monthly Notices, Vol. XXXIII. No. 3. 8vo. 1873.
*Bavarian Academy of Sciences, Royal.* 1870. Sitzungsberichte. 1872: Heft 2. 8vo.
Inhaltsverzeichniss, 1860–70. 8vo. 1872.
*British Architects, Royal Institute of*—Sessional Papers, 1872–3. Nos. 5, 6. 4to.
*Chemical Society*—Journal for Feb. 1873. 8vo.
*Comitato Geologico d' Italia*—Bolettini. 1872. Nos. 11, 12. 8vo. Firenze.
*Dawson, Principal J. W. F.R.S. (the Author)*—The Story of the Earth and Man. 16to. 1873.
*Editors*—American Journal of Science for Feb. 1873. 8vo.
Athenæum for Feb. 1873. 4to.
Chemical News for Feb. 1873. 4to.
Engineer for Feb. 1873.
Food Journal for Feb. 1873. 4to.
Journal for Applied Science for Feb. 1873. Folio.
Journal of Gas-Lighting for Feb. 1873. 4to.
Nature for Feb. 1873. 4to.
Nautical Magazine for Feb. 1873. 8vo.
Once a Week for Feb. 1873. 8vo.
Pharmaceutical Journal for Feb. 1873. 8vo.
*Franklin Institute*—Journal, Nos. 564, 565. 8vo. 1873.
*Geographical Society, Royal*—Correspondence respecting the New Arctic Expedition. 8vo. 1873.
*Geological Society*—Quarterly Journal, No. 113. 8vo. 1872.
*Iron and Steel Institute*—Journal, No. 4. 8vo. 1872.
*Macpherson, John M. O. M.R.I.*—J. M. Joule; Das Mechanische Wärmeäquivalent. Uebersetzt von J. W. Spengel. 8vo, Braunschweig, 1872.
*Manchester Literary and Philosophical Society*—Proceedings. Vol. XII. No. 6. 8vo. 1873.
*Mechanical Engineers' Institution, Birmingham*—Proceedings, 1872: July, Part 2. 8vo.
*Meteorological Office*—Quarterly Weather Report. 1872, Part 2. 4to.
Report of the Meteorological Conference at Leipzig. 8vo. 1873.
*Meteorological Society*—Quarterly Journal, No. 5. 8vo. 1873.
*Preussische Akademie der Wissenschaften*—Monatsberichte, Nov. 1872. 8vo.
*Royal Society of London*—Proceedings, No. 141. 8vo. 1872.
*Symons, G. J. Esq. (the Author)*—Symons' Monthly Meteorological Magazine, Feb. 1873. 8vo.

## WEEKLY EVENING MEETING,

Monday, March 7, 1873.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

James Dewar, Esq. F.R.S.E.

### *On the Temperature of the Sun and the Work of Sunlight.*

[Abstract deferred.]

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## TESTIMONIAL TO DR. BENCE JONES.

At a Meeting of the Managers of the Royal Institution, held on Monday, March 10, 1873, especially to consider a Letter from Dr. Bence Jones, resigning his office as Honorary Secretary, the following Resolution was unanimously agreed to:—

"The Board of Managers receive with deep regret the letter of Dr. Bence Jones containing his resignation of the office of Secretary to the Royal Institution.

"They lament the reason which compels him to relinquish a post which he has filled for so many years with equal honour to himself and advantage to the Institution.

"His conviction of the value of original research, and of the special vocation of the Royal Institution to continue diligent in promoting it, was with him an unceasing stimulus to exertion. His attention to every detail left nothing neglected in the performance of his duties. His own scientific attainments have been of signal effect in maintaining respect for the Institution, and in procuring the cooperation of eminent men in the Laboratories and Lecture Theatre. His love of the place and its memories has been shown by the pains he took to collect its early annals; including in this work an account of the discoveries of Young and Davy; and by his becoming the historian of Faraday.

"The Managers cannot fail to remember that the services of Dr. Bence Jones have been given under the pressure of important professional engagements, and latterly under the additional difficulties of failing health; and that until now, reluctantly compelled to resign, he has never relaxed in the active prosecution of his honourable task.

"The Managers, however, trust that the aid of Dr. Bence Jones may not be altogether lost to the Institution; but that he will still afford to it the benefit of his counsels and experience. They hope that he may in future occupy a seat at the Board of Management; and further, that he will remain associated with the Institution, by doing it the favour of accepting the position of Honorary Assistant-Secretary. They believe that the distinguished success which has attended Dr. Bence Jones in his career as Secretary will always be his worthiest reward. It rests with them to tender to Dr. Bence Jones the expression of their strong personal regard, and of their warmest gratitude for his invaluable services to the Royal Institution."

It having been ascertained that the form of Testimonial most agreeable to Dr. Bence Jones would be a Bust of himself to be placed in the Royal Institution, the Managers resolved to open a Subscription List among the Members of the Royal Institution for the purpose; Individual Subscriptions to be limited to 3*l*. 3*s*. as a maximum.

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## WEEKLY EVENING MEETING,

Friday, March 14, 1873.

Sir Henry Holland, Bart. M.D. F.R.S. President, in the Chair.

### Professor Allman, F.R.S.

### *On Coral Islands and their Architects.*

The speaker commenced by giving an account of the structure and habits of the *Actinia*, or sea-anemone, as a type of the coral polype. He described it as a fleshy sac attached by a broad base at one end, and having at the opposite end a mouth surrounded by a wreath of tentacles or feelers. He showed that its stomach consists of a smaller sac suspended in the larger, and opening at one end by the mouth, while at the other end it opens into the surrounding cavity of the larger sac. This free opening of the stomach into the general cavity of the body is a character of great importance, and is possessed by no other group of animals than that to which the sea-anemone belongs.

The group so constituted is named *Cœlenterata*, a designation derived from the Greek, and intended to express the peculiarity of structure here referred to. This curious open stomach is kept in its place by a set of vertical fleshy plates, which radiate from it to the surrounding walls of the great cavity of the body. Now, the various parts of the sea-anemone are under the control of a definite Number, by which its symmetry is regulated. This is the Number Six; and the radiating plates just mentioned are thus either six in number or some multiple of six, and so also the tentacles when freely and normally developed are either six or one of the multiples.

While the sea-anemone is immersed in the waters of the sea, and is surrounded by the conditions which promote its health and well-being, it will display its beautiful crown of tentacles and spread itself out like an expanded flower, often brilliantly and beautifully coloured. But let danger threaten, let a finger rudely touch it, and it will instantly draw in its tentacles and contract itself around them; and then, instead of the expanded flower, we may believe we have before us the same flower in the bud.

The speaker then pointed out how *Caryophyllia*, which may be found upon various parts of our own shores attached to rocks at low spring-tides, and which essentially agrees in structure with the sea-anemone, has the power of separating carbonate of lime from the sea-water and of depositing this mineral, particle by particle, in its tissues so as to become in great part calcified. In other words, how it has the power of forming a true skeleton of coral.

Now the *Caryophyllia* with its skeleton of coral, instead of remaining simple and solitary like the sea-anemone, may throw out buds like a plant, or may complicate itself by partially dividing into two or more segments, and will thus be converted into a compound colony of coral-producing polypes, each polype having its own mouth and stomach and tentacles and radiating plates; and each, while providing for its own wants, contributing at the same time to the general well-being of the colony. The form of this colony will depend on the disposition and mode of growth of the buds, or of the new polypes produced by the splitting of the old ones; for if all these remain closely impacted together, there would result such massive corals as are familiar in *Astræa* and *Meandrina;* while if they become more or less separated from one another and grow out into branches, we should then have a branched coral like a *Dendrophyllia* or a Madrepore. Now it is such animals as these Astræas and Meandrinas and Dendrophyllias and Madrepores which form the reefs and coral islands to whose history the discourse is devoted.

It must not, however, be supposed that all coral animals are reef-builders. The well-known red coral of commerce, for example, never accumulates in reefs or banks. This coral also, as well as many other non-reef-building kinds, differs in some subordinate points of structure from the true reef-builders; and instead of having its symmetry controlled by the number Six, it is the number Four by which the disposition of all its parts is regulated.

Having thus given an account of the nature of the coral animal and its mode of forming coral, the speaker proceeded to describe the home of the reef-builders. This region forms an irregular zone, extending for some distance at each side of the equator, but never going farther from it than about 30° of latitude. There can be no doubt that this coral zone is limited by temperature, and it has been pointed out by Dana, on what appears to be good evidence, that its northern and southern boundaries are formed by the isocrymal lines of 68°, or those lines which may be drawn through the parts of the ocean where the mean temperature of the coldest month of the year is 68° Fahr. These isocrymal lines, however, greatly deviate from the parallels of latitude, for while in some places they are separated from the equator by 30° of latitude, in other places they are strongly pressed towards it by the influence of the cold currents from the poles.

The extent of the coral formations within this area has ever since the seas began to be explored struck the navigator with astonishment. Along the western coast of New Caledonia is a reef of 400 miles in length, and along the north-east coast of Australia is one of more than 1200 miles; while it is to the labours of the reef-building polypes that almost all the beautiful islands which stud the tropical portion of the Pacific, and many of those in the Indian Ocean, are mainly due.

The speaker then drew a picture of the aspect of the ocean, and of the most striking members of its fauna in the region of coral reefs. He directed especial attention to the most superficial zone of the tropical ocean—that zone where sea and air and heat and light combine and concentrate the conditions of intensest animality, amid which becomes developed a most beautiful and marvellous fauna; where Medusæ and Siphonophores wander at their own wild will, propelled through the clear waters by the pulsations of crystal bell or of broad many-coloured disc, or in vast fleets are floating over the sea with sail extended to the breeze; where Pteropods flit on wings through the water like butterflies through the air; where Beroes catch the sunlight on their sides and flash it back in all the brightest hues of the rainbow; where Salpæ play in long undulating chains of crystal, and Pyrosoma, no less clear and crystal-like by day, becomes a cylinder of fire by night. What exuberance of life! What intensity of happiness! What unnumbered hosts basking beneath the tropic sky, or breaking the mirror of the sea with their gambols, or yielding to the impulse of the gentle trade-wind, or lighting up at night with phosphorescent gleam the dark waters of the deep!

Now, the coral-builders share with these bright and active hosts the prolific surface zone of the ocean. But their area is also a deeper one, and for many a fathom downwards their flower-like discs and banks of living coral may be traced by the sounding-lead and the dredge.

This extension downwards, however, is not unlimited. The ocean varies with its depth in its physical characters and in the conditions of life which it presents, and the coral-builders find the conditions suited to their welfare within a limited and definite range. This range never extends beyond a depth of from 20 to 30 fathoms, so that no living reef-building coral is ever found at a greater depth than this.

Within the upper portions of this area the coral fauna may be witnessed in all the perfection of its highest development and life; for there the sea is transparent as the purest beryl, and down fathoms deep below the boat the eye can penetrate through the liquid crystal to where the coral bank spreads around, and a scene of marvellous beauty becomes revealed. For there beneath the sea is a garden: carnations and asters and anemones and gorgeous cactus-flowers seem there to expand their glowing petals; flexile shrubs root themselves in the crevices of the rocks, and envelop their branches in bright clusters of flowers like the mezereon bush in the month of March. What profusion of forms! what richness of colouring! crimson, and golden, and purple, and emerald-green, and snowy white;—no garden of the upper air can surpass that garden of the sea in loveliness. But, stranger than all! every petal is replete with sense; every flower and every shrub is an animated being;—touch it, and it shrinks; feed it, and it digests; it rejoices in the warm sunlight, and feels happy in the caress of the ocean tide.

Now the animated flowers of that wonderful sea-garden spend no life of idleness—day after day, night after night, they are at their work! they are the builders of coral, the architects of islands, the ceaseless labourers by whose untiring energy have been rescued from the ocean thousands of miles of habitable land.

Having thus examined the workers, and become acquainted with their home, the speaker proceeded to consider the nature of their works, and this subject would be best treated under the following heads:—

1. The forms and structure of the reefs.
2. The mode of their construction.
3. Their relations to man.

### *The Forms and Structure of Coral Reefs.*

Coral formations have been divided into three classes: the Atoll, or Lagoon Island, the Barrier Reef, and the Fringing Reef.

The Atoll is the type of the Coral Island. It presents the appearance of a circular or irregularly formed ring of coral rising out of the bosom of the ocean, generally clothed with a rich tropical vegetation of cocoanut palms, pandanus, and pisonia, surrounded by a wreath of white foam where the sea breaks upon its outer margin, and having a lagoon or lake of still water in the interior.

The ring of coral is usually discontinuous in one or more places, and through the channels thus formed ships can generally sail into the calm central lagoon, which will then afford them, no matter how rough may be the external sea, a safe and commodious harbour.

It is scarcely possible to conceive of anything more lovely than one of these Lagoon Islands, with its graceful palm-trees and its groves of pisonia, the still quiet lake within, the restless landless ocean without, and the glowing sky of the tropics stretching over all. Where

> "Droops the heavy blossomed bower; hangs the heavy fruited tree,
> Summer isles of Eden lying in dark purple spheres of sea."

Some of the small uninhabited Atolls especially seem almost as if they had been the work of enchantment. Even the wild creatures which dwell there appear to realize the wonders of the fairy tale and make us almost believe that they lie under the spell of a magician. The United States' explorers assure us that in some of these islands the birds were so little alarmed at the presence of man, that they allowed themselves to be taken off the branches of the trees as if they had been their flowers.

When the Atoll is examined more minutely, it is found that the depth of the central lagoon may be about ten or twenty fathoms; a pure coral sand or fine white coral mud covers its bottom; the flexible and shrub-like species of coral root themselves in its sides, and millions of strange creatures nestle in their branches, or creep over the coral rock, or dart through the waters of the lagoon.

On the outer side of the ring of coral things are very different. The reef here usually extends outwards for some distance as a flat platform, with but a slight depth of water over it; the depth then suddenly increases, and immediately afterwards the sounding-lead sinks into the fathomless ocean where no bottom can be felt. One of the most remarkable features in an Atoll is thus the extraordinarily rapid rate at which its outer side sinks into the ocean; and this fact must especially be noted, for it will aid us in our attempts to explain the formation of the island.

When masses of the coral of which the reef is composed are brought up from various depths, between the surface and twenty or thirty fathoms below it, these fragments are generally found to be alive. From much greater depths, however, the sounding-lead will continue to bring up fragments of coral which it has detached from the sides of the reef; but whatever fragments are brought up from greater depths than at most thirty fathoms are invariably found to be dead. This fact is in accordance with what has been already said regarding the limited range in depth at which the coral animal can live; and it is another very important one, which must be also kept in mind as necessary in enabling us to understand the mode of formation of the island.

If a correct idea of the nature of an Atoll be now conveyed, there will be no difficulty in understanding the two remaining classes of coral formations.

The Barrier Reef consists of a ridge of coral resembling that of an Atoll, but running parallel to the shores of a continent, or surrounding those of an ordinary island, and in either case at such a distance as to include between it and the land a deep channel of still water.

A Fringing Reef differs from a Barrier Reef only in being smaller, and scarcely including any channel between it and the land, which it thus merely fringes with a skirt of coral rock.

### *Mode of Formation of Reefs.*

When it was first known that so many of the islands in the Pacific and Indian Oceans owed their existence to the energy of coral animals, it was believed that the coral-formers commenced their labours upon the flat bottom of the sea, at unknown and unfathomable depths, and that they thence worked continuously upwards to the surface. This hypothesis, however, is inconsistent with the now well-established fact of the limited range in depth of living coral.

The circular form of the Atoll suggested also another hypothesis, which maintained that the coral animals established themselves round the crater of some submarine volcano, and that, thence working upwards, they necessarily repeated on the surface of the sea the circular form of their foundation. Now this hypothesis would involve the highly improbable supposition that there exists throughout the region of corals innumerable volcanic craters, and that all these are nearly at the very same level, namely, that which alone is suited to the energies of the living coral; while its improbability is rendered still further apparent by the fact that there is no known volcanic crater whose diameter approaches in dimensions to that of many Atolls.

For the beautiful theory which is now universally accepted—the only one, indeed, which is consistent with all the known phenomena, and one of the most important with which the physical history of the earth has of late years been enriched—we are indebted to Mr. Darwin.

The theory of Darwin is founded on two incontestable facts,—the one purely physiological, the other purely physical.

The physiological element in the theory of Darwin consists in the fact already insisted on, that the coral animals cannot live at unlimited depths, that they need the presence of light and of other conditions which they can obtain only near the surface, and that from twenty to thirty fathoms below the surface is the greatest depth at which the reef-building polypes can continue to live.

But we have also seen that the outer walls of the coral reef have been followed downwards into depths very much greater than this; and though the coral forming those deep portions is always found to be dead, it is plain that it must have been at one time living, and some additional fact is therefore needed in order to reconcile its occurrence at these great depths with the established limited range of living coral. Here, then, comes to our aid the physical element in the theory. It is this: That while the ocean maintains the same level from age to age all over the world, the solid land is subject to repeated oscillations of level, rising in one place and sinking in another, and this sometimes to an extent of many thousands of feet.

Having demonstrated the reality of this phenomenon by reference to numerous well-known geological facts, such as the sinking of the southern shores of Scandinavia and of the western shores of Greenland, and the rising of the northern shores of Scandinavia and of Siberia, the speaker proceeded, with the aid of diagrams, to apply, as Darwin had done, the two groups of phenomena, physiological and physical, to the explanation of coral formations.

He showed how a mountain, rising out of the sea in the form of a precipitous island, in the region of the reef-builders, will present on its shores the conditions suited to the coral-polypes, which will there attach themselves, building downwards until they arrive at depths too great for the perfect exercise of their functions, and upwards until the surface of the sea sets bounds to the further elevation of their structures. A reef of coral will thus be spread all round the shores of the island, and will constitute the formation known as a Fringing Reef.

But the island is supposed to be in a region of subsidence, and has begun to sink slowly into the sea, carrying with it the already-formed fringe of coral into depths incompatible with the well-being of the polypes. Urged, however, by an unerring instinct, the reef-builders continue their labours upwards simultaneously with the gradual depression of the land, and thus the reef is always extending itself towards the warm sunlit surface of the sea, where all the conditions of coral life exist, while the lower parts have passed downwards into depths where the formers of coral must cease to live.

The reef has thus grown larger; and as the coral is produced in greater force on the outer edge, where the reef is exposed to the open ocean, with all the conditions in which the animals forming it delight, this part is sooner brought to the surface than the inner part, where the growth of the coral is still further interfered with by the accumulation of fragments which the waves tear from the reef, break down into coral sand and coral mud, and carry inwards towards the land. A deep channel is thus formed between the outer part of the reef and the shores round which the coral has attached itself; and what was at first a Fringing Reef becomes in this way converted into a Barrier Reef.

In the meantime fragments of coral broken off by the waves are gradually piled upon the upper surface of the reef, which is thus in time raised above the sea in the form of a long stretch of dry land, separated from what still remains of the original island by the intervening channel of still water, and capable in the course of time of affording, by the decomposition of its surface, a soil in which terrestrial plants may take root.

But the changes do not end with the formation of a Barrier Reef; for the work of subsidence goes on, and the ancient land continues to sink deeper and deeper into the sea, carrying the coral-polypes down with it into the dark, ungenial ocean depths, where they must inevitably perish. And now at last the highest point has disappeared, all has sunk beneath the sea, and a wide waste of landless waters rolls unbroken over its summit.

The island architects are not, however, to be baffled. As the lower parts of the reef sink into depths, where they must perish, the upper parts are simultaneously extending themselves as a bank of living coral towards the surface, which at last they reach in the form of a more or less circular reef, on which the waves once more break, and which includes within it a sheltered lagoon, now free from even the last remnant of included land; and the Barrier Reef becomes thus converted into an Atoll.

Now it will be here noted that throughout the whole of the changes which have been thus traced, the thickness of the bed of living coral is a constant quantity, extending always from a little below the surface of the sea to that fixed plane beyond which if the coral be carried it ceases to live, while, on the other hand, the mass of dead coral is continually increasing with the subsidence of the land.

It is also evident from what has now been said that the Atoll points out the spot where an ordinary island had become submerged, and that the whole region of Atolls and Barrier Reefs has been gradually subsiding. A subject of great significance is thus suggested by the study of the phenomena of Coral Reefs; and the conclusion is irresistibly forced upon us that in the region where now the Pacific Ocean separates the Old World from the New there lay in former times a continent with its mountain peaks and table-lands; that all has since sunk beneath the sea, except its highest table-lands and the summits of its highest mountains; that within the region of the reef-builders the coral has gathered in its encircling reefs round these last remnants of the ancient land; while an Atoll marks the spot where a mountain peak of that old continent has totally disappeared beneath the ocean.

In those regions which lie beyond the warmer area of the coral, the land may yet have sunk and left no sign; for the reef-builders could not live in the colder waters, and there was nothing else to tell the tale.

## *The Coral Island in its relations to Man.*

But the Atoll whose formation we have been following is not yet dry land. It is still a submerged reef over which the waves roll, for the polypes cannot extend their works into the upper air. Further changes therefore still await it. Fragments torn from its outer side by the waves are piled upon its surface, and it rises higher and higher from the sea; the decomposing coral covers the reef with a fertile soil, to which the wind and the ocean currents may bring the seeds of plants from other lands, and a graceful vegetation clothes in time its sea-girt shores.

And thus the Atoll becomes fitted for the sustenance of terrestrial animals and man. Sea-birds in multitudes find shelter there, and land-birds from distant shores see in it a country where they may dwell; while the drifted trunk of the forest tree, to which the lizard and the insect still cling, is cast upon its strand to begin the peopling of its woods with still other forms of life.

Some large fruit-eating bats too have discovered it; but no other mammal has ever formed part of the aboriginal fauna of the Coral Island.

Its latest occupant is doubtless man; but whence he came, from what original stock he migrated, we have no positive evidence to determine.

If we except the Feejee group and some of the other high coral-encircled islands which lie at the extreme west of the area, and in which the inhabitants are Negrittos, characterized by their black skins, frizzled hair, and repulsive features, we shall find a great uniformity of type to prevail over the rest of the Pacific coral region, a type with lighter skin and straight or wavy hair, and one which points towards an affinity with the Malay races of south-eastern Asia. We are not, however, on this account to suppose that the islands were necessarily peopled by direct migration from the Asiatic shores. If we suppose that the high islands, with their encircling barrier reefs, represent the last remnant of a submerged continent, it is quite possible that these islands may have retained the last remnant also of its population. But whether this be so or not, it is certain that the Atolls must have been independently peopled, either from the surrounding islands or from some more distant continent. The Atoll rose from the bosom of the ocean destitute of terrestrial life, and many a century must have passed before the presence of man broke the solitude of its shores. We may well believe that at last some savages, drifted out of their course by adverse winds, had run their canoe upon the strand and had taken possession of the land of the polype. The fish of the lagoon, the mollusca of the shore, the fruits of the wood, afford them their subsistence; they have discovered that a pit dug a few feet deep into the coral rock will reach a reservoir of fresh water accumulated from the rains which had percolated through the more superficial porous structures; the friction of two pieces of dry timber yields them fire; while their spears fashioned by a piece of sharp stone found entangled in the roots of some driftwood, facilitate the acquisition of food, and make the struggle for existence more easy.

And so years pass on, and the descendants of the first accidental settlers have peopled the island; but though experience may have taught them many things, though an imperfect division of labour may have been adopted and a rude state of society established, the aboriginal dwellers have rather adapted themselves to the physical conditions among which they have been thrown than raised themselves above them, and centuries pass away and leave them little changed. For after all, the Coral Island is but ill fitted for human development; without a hill to break the uniformity of its surface, without a stream to hollow out a valley, without a single metal, without a mineral beyond the unvaried calcareous coral rock, without a mammal other than the bird-like bat, the native, cut off from all communication with other lands, will have few ideas; while his wants, few and easily satisfied by the spontaneous produce of the island, will afford little motive for exertion, and little stimulus to development; and it is later times, and other lands where civilization has already spread, where knowledge has already advanced, that must carry civilization and knowledge to the Coral Island.

And so centuries still roll on, until at last from these other shores the destined race has landed on the island—a race with higher powers, a more cultivated intellect, and an increased capacity for improvement—the bearer of a new force, of a civilization with all its good, and, alas! with all its evils too: but the good is greater than the evil, and so the lower yields to the higher phase of intellect; more perfect social relations are introduced; laws are instituted; the savage rites of a degrading superstition give place to a more elevated and a purer creed; no longer with that narrow policy which does not look beyond the limits of its own strand of coral, a common interest has united island to island, and a national life has dawned upon the archipelago of the polype; deficiencies are supplemented by the products of other lands; the breadfruit and the banana and the yam are cultivated; the fruits of the woods and the pearl-shell and the trepang of the lagoons are sought after by the trader; the relations of commerce are established, and the Coral Island takes its place in the great community of Nations.

[G. J. A.]

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## WEEKLY EVENING MEETING,

Friday, March 21, 1873.

William Spottiswoode, Esq. LL.D. Treas. R.S. & R.I. Vice-President, in the Chair.

### Captain E. D. Lyon

### *On the Mythology and Temples of India,*

Illustrated by Photographic Views of the Temples.

India is the brightest appanage of the British crown, containing as it does about one hundred and forty millions of inhabitants; and yet how little is known about it. Most persons have heard of an Indian temple or pagoda, but such words are not known in India, the word used being *Devila*, which means "the house of God," and what English people call a pagoda is nothing but the door or entrance to the building of God. The difficulty of obtaining information on the subject of the Hindoo religion in India is very great. The Anglo-Indians are far too much occupied to trouble themselves on such matters; and the only persons of whom inquiries can be made are the Brahmins and the missionaries. With the former, experience teaches that the application of silver to their hand has a wonderful effect on their tongue, and if you only apply enough coin you may get any amount of information; but then comes the difficulty of separating what is true and what is imaginary. As to the missionaries, it is hardly likely they will take the trouble to study much a religion which they have come to try and root out. One of them declares that in India the natives worship three hundred and thirty millions of gods; but the real truth is, the Hindoos worship only one supreme Deity, whom they look on as too sublime, too grand, too awful for their minds to conceive. They never address any prayer to him; they have never erected any temple to his honour, and they would consider it sacrilege to do so. All their prayers are addressed to one of his triune personifications, Brahma, Vishnu, and Shiva. Brahma personifies creation; Vishnu, preservation; and Shiva, destruction. These three agree in one; for though at first sight it might appear difficult to reconcile creation and destruction as agreeing; still when it is remembered that with the Hindoos destruction is in reality re-creation, the coalition is easy. In former times the worshippers of the three divinities also agreed and used the same forms of worship and the same temples, as is proved by the older temples containing close together representations of the three deities; but of late years they hate each other with as fanatical a hate as is to be found in the Western world.

Captain Lyon then began the exhibition of his photographs, enlarged by the camera, and illuminated by the electric light; first introducing the god Ganesa, one of the most noted in India, and the one which every Hindoo prays to before commencing any undertaking. Before he begins a journey, before he builds a house, he prays to Ganesa—"O glorious and immortal Ganesa, grant me success in what I am about to undertake!"

"I have already mentioned," said Captain Lyon, "the Indian temple or house of God. I must now explain that the temple is composed of four parts: the Gopurum, or gateway, much resembling the Egyptian Pylon; the Mundapum, or porch; the Teppa Kolum, or tank; and the Vimanum, or sanctuary. The three first I will show you; the last is a low oblong room, perfectly plain, into which no ray of daylight is ever permitted to enter. It is here the idol is kept, and no one but a high-caste Hindoo and a royal prince is allowed to enter; and if a prince goes in he must go quite alone.

"The first temple of which I shall show you a view, is that of Seringum, near Trichinopoly. It is one of the largest in India. You see the Gopura which mark the entrances from one court to another. The base is of stone, and the pyramid of brick covered with plaster, out of which all the figures are made. There are twenty-three of these Gopura in this temple, and eight thousand Brahmins live in it; but you must not suppose all the Brahmins are priests. It is from among the Brahmins that the priests are chosen; as among the Jews it was from the tribe of Levi the family of Aaron was chosen for the priesthood.

"A mundapum, or porch, of which literally thousands are to be found in India, is composed of eight stones, say eight feet long and about two feet square. Take four of these and put them upright in the ground, lay the other four along the top to form a roof, and you have a mundapum; and whether of immense size and beautifully carved, or quite plain, the shape and form are the same.

"In the view before you each group of horses, animals, and figures are cut out of one block of granite, fifteen feet high, and it forms the front of one of the mundapa in the Seringum temple. The Teppa Kolum, or tank, which you see in this view, is also in the Seringum temple, and is the third part of a Hindoo temple, as already mentioned. At a festival in January the god is put on a boat, and drawn twenty-one times round the pavilion which you see in the middle, the Nautch girls dancing and the band playing.

"Madura, the Rome of the Hindoos, is the next place to visit. Here we find the celebrated Puthu Mundapum, built by Trimul Nayak, the last king of Madura. He asked the god to come and pay him a visit; and the answer was, he had no house fit to receive him. The king erected this wonderful building, which cost one million sterling, and took twenty-two years to complete. It is 333 feet long, by eighty-one broad. The roof is supported by 128 pillars, each of which is a solid block of granite twenty-five feet high, and all most beautifully sculptured.

"You have heard of the Juggernaut car, which is the next view. The god, besides his row on the tank, must have his car in which he is drawn about on a certain day every year. The whole population of the neighbouring villages assembles; the god is brought out, covered with jewels, and being carried to the top of the car by means of an inside staircase, the enormous ropes are attached, and being seized by thousands of men, the ponderous mass begins slowly to move; and what with the shouting of the men, the singing of the women, and the dancing of the Nautch girls, the scene is one never to be forgotten. The excitement becomes at last so intense that one hardly wonders that the fanatics become absolutely mad, and that it is with difficulty the police, appointed by the government for the purpose, can restrain them from throwing themselves on their backs on the ground, and opening their legs permit the wheels of the machine to pass over their bodies, welcoming so horrible a death in the belief that it will ensure to them beatitude hereafter.

"Farther south we shall see the Monolith at Kaloogoomulla, which not six white persons have ever seen, and very few know even of its existence. For some reason the Brahmins will not tell anyone of its whereabouts; and when questioned deny all knowledge of it. It is cut out of a solid block of dark granite, and stands perfectly isolated. No worship of any sort is ever performed there, and it is not known when or by whom it was erected or for what purpose.

"At Tennevelly, yet farther south, are four gigantic figures, each cut out of a solid block of a dark green stone, said to be a mixture of trapstone and hornblend, twenty feet high. The one you see is the Kama Deva, or Indian Cupid, who was born with a bow and five arrows; and on asking Brahma, his father, for what purpose these were given to him, was told that to him was given the power to inflame mankind with love, and thus people the world. He thought he would try his arrows first on Brahma, and the result was he was soon blest with 149,000 brothers and sisters.

"The sacred temple of Ramisseram on the island of Paumben, situated at the commencement of what is called Adam's bridge, which is said formerly to have joined Ceylon to the mainland. The story of the sanctity of this island and temple is to be read in the 'Ramayana,' a Hindoo poem filling a very large volume; being a history of the battles which took place between Rama and Ravana, when the latter, a monster with ten heads and one hundred arms, carried off Sita, Rama's wife. The temple is the most sacred in all India, not even excepting Benares. It is here the pilgrim ends his long journey which he began three thousand miles away; and no one who does not know what Indian travel is, can realize what the pilgrim suffers in this tedious march, without mentioning the austerities to which he condemns himself—such as prostrating himself, face downwards, in the dust at the end of every mile. Here is rest at last; and as he wanders in the waning light through these sacred precincts, he forgets all he has undergone in the remembrance that he has gained his goal, and he looks forward to blessings in this world and immediate beatitude after death. The wonderful corridors of the temple itself are justly considered one of the most marvellous exhibitions of human labour to be found anywhere; their aggregate length is nearly four thousand feet; and when we consider that their section is that of a small Gothic church, and their length eight times that of our largest cathedral, that all the pillars are of granite richly carved, and observe the variety of light and shade, and the wonderful effects of perspective in the long-drawn aisles, we must admit that they form an *ensemble* surpassing anything of its class to be found elsewhere.

"In Avadea Covill we find again the hard stone, as at Tinnevelly, which no chisel in these modern days can be made to cut. The temple is small, but wonderfully beautiful. At Tanjore the temple is one of the finest and richest in India, but, curious to say, almost entirely deserted: it is celebrated for the large stone bull Nundi, which our engineers compute must have weighed nearly one hundred tons before it was cut, and must have been brought five hundred miles, as there is no quarry of a similar stone nearer.

"At the Seven Pagodas, near Madras, are the extraordinary rocks which have been so beautifully carved, both inside and out; but by whom, when, or for what purpose, is not known. The whole undertaking seems to have been begun at once and ceased suddenly. The bassi-relievi round the sides of the hollowed-out rocks are as fine, if not finer, than anything to be found in India. There also is the temple on the sea-shore, supposed to be all that remains of the submerged city of Bali, the wildest fiction that ever was invented; but nevertheless immortalized by Southey in his 'Curse of Kehama.' Close by are the Five Raths, as they are called, large boulders cut into beautiful little temples, said to be the only Buddhist temples in India, the other remains being all caves as at Karlee, where we see one of the finest of them; and curiously enough, the wood-work, both at the entrance and also at the farther end, date from the time of the original excavations, probably a few years before our Saviour. The shape of the interior is very similar to that of the early Christian churches, the Dagoba being in the place of the altar. At Tarputy, 140 miles north of Madras, we find again the same stone as at Avadea Covill and Tinnevelly, and here even more marvellously sculptured. The bases of what were splendid Gopura, but now in ruins, are covered with a profusion of beautifully-executed figures, flowers, and animals, such as a Hindoo could alone have attempted. Next comes the city of Vijianuggur, which formerly covered seven square miles, and which has now almost entirely disappeared, leaving only two small temples as records of its former grandeur. This city defied for two hundred years the power of the Mahomedans, whose capital, Beejapoor, is very interesting to visit on account of the tombs of its kings, which alone in like manner remain to attest its magnificence. In this city each king built his own tomb during his lifetime; a plan which had the one disadvantage, that if the king died before it was half finished it remained in the same state for ever. The verandah round the mausoleum of Ibrahim Shah is as fine as anything to be found in the East; and, when the azure colouring was fresh, the contrast between it and the gilt letters and the outer frame of the grey granite, must have been grand indeed. The whole of the Koran has been engraved on the walls of this tomb. Over the windows the stone has been pierced, and the Arabic letters alone left, which are so well done and perfect as to be easily legible by anyone even imperfectly acquainted with the language. Whether this marvellous exhibition of skill and patient industry, or the window of the mosque at Ahmedabad, or the screen round the Taj at Agra, bears off the palm, is a question very hard to answer."

In conclusion, Captain Lyon devoted a few words to the Jain religion: "There can be little doubt that the founder of this religion, Mahavira, was the preceptor of Buddha, and that the cause of their separation was that Mahavira tried to persuade his disciples that they should go about in a state of nudity. Buddha saw at once that such a precept could not practically become popular, and the result was their quarrel, and Buddha became the founder of the religion which, even at this day, numbers more disciples than all the religions of the West, while the Jains for a time seemed almost to have disappeared, to reappear in small numbers again, when the Buddhists were driven by the Brahminical faith out of India. There are three or four small colonies of them now scattered about, and at Bombay they are very rich, and have built on a hill they have purchased in Goozerat some very beautiful temples. They are a quiet and very hard-working race of men, and their religion is one of the very best in India; their Bible, the Kalpa Sutra, being as practically good as the Koran."

[E. D. L.]

# Royal Institution of Great Britain.

## WEEKLY EVENING MEETING,

Friday, April 4, 1873.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

Professor Tyndall, LL.D. F.R.S.

### *Some Observations on Niagara.*

It is one of the disadvantages of reading books about natural scenery that they fill the mind with pictures, often exaggerated, often distorted, often blurred, and even when well drawn injurious to the freshness of first impressions. Such has been the fate of most of us with regard to the Falls of Niagara. There was little accuracy in the estimates of the first observers of the cataract. Startled by an exhibition of power so novel and so grand, emotion leaped beyond the control of the judgment, and gave currency to notions regarding the waterfall which have often led to disappointment.

A record of a voyage in 1535 by a French mariner named Jacques Cartier, contains, it is said, the first printed allusion to Niagara. In 1603 the first map of the district was constructed by a Frenchman named Champlain. In 1648 the Jesuit Rageneau, in a letter to his superior at Paris, mentions Niagara as "a cataract of frightful height."* In the winter of 1678 and 1679 the cataract was visited by Father Hennepin, and described in a book dedicated "to the King of Great Britain." He gives a drawing of the waterfall, which shows that serious changes have taken place since his time. He describes it as "a great and prodigious cadence of water, to which the universe does not offer a parallel." The height of the fall, according to Hennepin, was more than 600 feet. "The waters," he says, "which fall from this great precipice do foam and boil in the most astonishing manner, making a noise more terrible than that of thunder. When the wind blows to the south its frightful roaring may be heard for more than fifteen leagues." The Baron la Hontan, who visited Niagara in 1687, makes the height 800 feet. In 1721 Charlevois, in a letter to Madame de Maintenon, after referring to the exaggerations of his predecessors, thus states the result of his own observations:—"For my part, after examining it on all sides, I am inclined to think that we cannot allow it less than 140 or 150 feet,"—a remarkably close estimate. At that time, *viz.* a hundred and fifty years ago, it had the shape of a horseshoe, and reasons will subsequently be given for holding that this has been always the form of the cataract from its origin to its present site.

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* From an interesting little book presented to me at Brooklyn by its author, Mr. Holly, some of these data are derived: Hennepin, Kalm, Bakewell, Lyell, Hall, and others I have myself consulted.

As regards the noise of the cataract, Charlevois declares the accounts of his predecessors, which, I may say, are repeated to the present hour, to be altogether extravagant. He is perfectly right. The thunders of Niagara are formidable enough to those who really seek them at the base of the Horseshoe Fall; but on the banks of the river, and particularly above the fall, its silence, rather than its noise, is surprising. This arises, in part, from the lack of resonance; the surrounding country being flat, and therefore furnishing no echoing surfaces to reinforce the shock of the water. The resonance from the surrounding rocks causes the Swiss Reuss at the Devil's Bridge, when full, to thunder more loudly than the Niagara.

On Friday, the 1st of November, 1872, just before reaching the village of Niagara Falls, I caught, from the railway train, my first glimpse of the smoke of the cataract. Immediately after my arrival I went with a friend to the northern end of the American Fall. It may be that my mood at the time toned down the impression produced by the first aspect of this grand cascade; but I felt nothing like disappointment, knowing, from old experience, that time and close acquaintanceship, the gradual interweaving of mind and nature, must powerfully influence my final estimate of the scene. After dinner we crossed to Goat Island, and turning to the right, reached the southern end of the American Fall. The river is here studded with small islands. Crossing a wooden bridge to Luna Island, and clasping a tree which grows near its edge, I looked long at the cataract, which here shoots down the precipice like an avalanche of foam. It grew in power and beauty as I gazed upon it. The channel, spanned by the wooden bridge, was deep, and the river there doubled over the edge of the precipice like the swell of a muscle, unbroken. The ledge here overhangs, the water being poured out far beyond the base of the precipice. A space, called the Cave of the Winds, is thus enclosed between the wall of rock and the cataract.

Goat Island terminates in a sheer dry precipice, which connects the American and the Horseshoe Falls. Midway between both is a wooden hut, the residence of the guide to the Cave of the Winds, and from the hut a winding staircase, called Biddle's Stair, descends to the base of the precipice. On the evening of my arrival I went down this stair, and wandered along the bottom of the cliff. One well-known factor in the formation and retreat of the cataract was immediately observed. A thick layer of limestone formed the upper portion of the cliff. This rested upon a bed of soft shale, which extended round the base of the cataract. The violent recoil of the water against this yielding substance crumbles it away, undermining the ledge above, which, unsupported, eventually breaks off, and produces the observed recession.

At the southern extremity of the Horseshoe is a promontory, formed by the doubling back of the gorge, excavated by the cataract, and into which it plunges. On the promontory stands a stone building, called the Terrapin Tower, the door of which had been nailed up because of the decay of the staircase within it. Through the kindness of Mr. Townsend, the superintendent of Goat Island, the door was opened for me. From this tower, at all hours of the day, and at some hours of the night, I watched and listened to the Horseshoe Fall. The river here is evidently much deeper than the American branch; and instead of bursting into foam where it quits the ledge, it bends solidly over and falls in a continuous layer of the most vivid green. The tint is not uniform but varied; long stripes of deeper hue alternating with bands of brighter colour. Close to the ledge over which the water rolls, foam is generated, the light falling upon which and flashing back from it, is sifted in its passage to and fro, and changed from white to emerald green. Heaps of superficial foam are also formed at intervals along the ledge, and immediately drawn down in long white striæ.* Lower down, the surface, shaken by the reaction from below, incessantly rustles into whiteness. The descent finally resolves itself into a rhythm, the water reaching the bottom of the fall in periodic gushes. Nor is the spray uniformly diffused through the air, but is wafted through it in successive veils of gauze-like texture. From all this it is evident that beauty is not absent from the Horseshoe Fall, but majesty is its chief attribute. The plunge of the water is not wild, but deliberate, vast, and fascinating. From the Terrapin Tower, the adjacent arm of the Horseshoe is seen projected against the opposite one, midway down; to the imagination, therefore, is left the picturing of the gulf into which the cataract plunges.

The delight which natural scenery produces in some minds is difficult to explain, and the conduct which it prompts can hardly be fairly criticised by those who have never experienced it. It seems to me a deduction from the completeness of the celebrated Thomas Young, that he was unable to appreciate natural scenery. "He had really," says Dean Peacock, "no taste for life in the country; he was one of those who thought that no one who was able to live in London would be content to live elsewhere." Well, Dr. Young, like Dr. Johnson, had a right to his delights; but I can understand a hesitation to accept them, high as they were, to the exclusion of

> "That o'erflowing joy which Nature yields
> To her true lovers."

To all who are of this mind, the strengthening of desire on my part to see and know Niagara Falls, as far as it is possible for them to be seen and known, will be intelligible.

On the first evening of my visit, I met, at the head of Biddle's

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* The direction of the wind with reference to the course of a ship may be inferred with accuracy from the foam-streaks on the surface of the sea.

Stair, the guide to the Cave of the Winds. He was in the prime of manhood—large, well built, firm and pleasant in mouth and eye. My interest in the scene stirred up his, and made him communicative. Turning to a photograph, he described, by reference to it, a feat which he had accomplished some time previously, and which had brought him almost under the green water of the Horseshoe Fall. "Can you lead me there to-morrow?" I asked. He eyed me inquiringly, weighing, perhaps, the chances of a man of light build and with gray in his whiskers in such an undertaking. "I wish," I added, "to see as much of the fall as can be seen, and where you lead I will endeavour to follow." His scrutiny relaxed into a smile, and he said, "Very well; I shall be ready for you to-morrow."

On the morrow, accordingly, I came. In the hut at the head of Biddle's Stair I stripped wholly, and re-dressed according to instructions,—drawing on two pairs of woollen pantaloons, three woollen jackets, two pairs of socks, and a pair of felt shoes. Even if wet, my guide urged that the clothes would keep me from being chilled; and he was right. A suit and hood of yellow oilcloth covered all. Most laudable precautions were taken by the young assistant of the guide to keep the water out; but his devices broke down immediately when severely tested.

We descended the stair; the handle of a pitchfork doing in my case the duty of an alpenstock. At the bottom my guide inquired whether we should go first to the Cave of the Winds, or to the Horseshoe, remarking that the latter would try us most. I decided to get the roughest done first, and he turned to the left over the stones. They were sharp and trying. The base of the first portion of the cataract is covered with huge boulders, obviously the ruins of the limestone ledge above. The water does not distribute itself uniformly among these, but seeks for itself channels through which it pours torrentially. We passed some of these with wetted feet, but without difficulty. At length we came to the side of a more formidable current. My guide walked along its edge until he reached its least turbulent portion. Halting, he said, "This is our greatest difficulty; if we can cross here, we shall get far towards the Horseshoe."

He waded in. It evidently required all his strength to steady him. The water rose above his loins, and it foamed still higher. He had to search for footing, amid unseen boulders, against which the torrent rose violently. He struggled and swayed, but he struggled successfully, and finally reached the shallower water at the other side. Stretching out his arm, he said to me, "Now come on." I looked down the torrent as it rushed to the river below which was seething with the tumult of the cataract. De Saussure recommended the inspection of Alpine dangers with the view of making them familiar to the eye before they are encountered; and it is a wholesome custom in places of difficulty to put the possibility of an accident clearly before the mind, and to decide beforehand what ought to be done should the accident occur. Thus wound up in the present instance, I entered the water. Even where it was not more than knee-deep its power was manifest. As it rose around me, I sought to split the torrent by presenting a side to it; but the insecurity of the footing enabled it to grasp the loins, twist me fairly round, and bring its impetus to bear upon the back. Further struggle was impossible; and feeling my balance hopelessly gone, I turned, flung myself towards the bank I had just quitted, and was instantly swept into shallower water.

The oilcloth covering was a great incumbrance; it had been made for a much stouter man, and standing upright after my submersion, my legs occupied the centres of two bags of water. My guide exhorted me to try again. Prudence was at my elbow, whispering dissuasion; but taking everything into account, it appeared more immoral to retreat than to proceed. Instructed by the first misadventure, I once more entered the stream. Had the alpenstock been of iron it might have helped me; but as it was, the tendency of the water to sweep it out of my hands rendered it worse than useless. I, however, clung to it by habit. Again the torrent rose, and again I wavered; but by keeping the left hip well against it, I remained upright, and at length grasped the hand of my leader at the other side. He laughed pleasantly. The first victory was gained, and he enjoyed it. "No traveller," he said, "was ever here before." Soon afterwards, by trusting to a piece of drift wood which seemed firm, I was again taken off my feet, but was immediately caught by a protruding rock.

We clambered over the boulders towards the thickest spray, which soon became so weighty as to cause us to stagger under its shock. For the most part nothing could be seen; we were in the midst of bewildering tumult, lashed by the water, which sounded at times like the cracking of innumerable whips. Underneath this was the deep resonant roar of the cataract. I tried to shield my eyes with my hands, and look upwards; but the defence was useless. My guide continued to move on, but at a certain place he halted, and desired me to take shelter in his lee and observe the cataract. The spray did not come so much from the upper ledge as from the rebound of the shattered water when it struck the bottom. Hence the eyes could be protected from the blinding shock of the spray, while the line of vision to the upper ledges remained to some extent clear. On looking upwards over the guide's shoulder I could see the water bending over the ledge, while the Terrapin Tower loomed fitfully through the intermittent spray gusts. We were right under the tower. A little farther on the cataract, after its first plunge, hit a protuberance some way down, and flew from it in a prodigious burst of spray; through this we staggered. We rounded the promontory on which the Terrapin Tower stands, and pushed, amid the wildest commotion, along the arm of the Horseshoe, until the boulders failed us, and the cataract fell into the profound gorge of the Niagara River.

Here my guide sheltered me again, and desired me to look up; I did so and could see, as before, the green gleam of the mighty curve sweeping over the upper ledge, and the fitful plunge of the water as the spray between us and it alternately gathered and disappeared. An eminent friend of mine often speaks to me of the mistake of those physicians who regard man's ailments as purely chemical, to be met by chemical remedies only. He contends for the psychological element of cure. By agreeable emotions, he says, nervous currents are liberated which stimulate blood, brain, and viscera. The influence rained from ladies' eyes enables my friend to thrive on dishes which would kill him if eaten alone. A sanative effect of the same order I experienced amid the spray and thunder of Niagara. Quickened by the emotions there aroused, the blood sped exultingly through the arteries, abolishing introspection, clearing the heart of all bitterness, and enabling one to think with tolerance, if not with tenderness, on the most relentless and unreasonable foe. Apart from its scientific value, and purely as a moral agent, the play, I submit, is worth the candle. My companion knew no more of me than that I enjoyed the wildness; but as I bent in the shelter of his large frame he said, "I should like to see you attempting to describe all this." He rightly thought it indescribable. The name of this gallant fellow was Thomas Conroy.

We returned, clambering at intervals up and down so as to catch glimpses of the most impressive portions of the cataract. We passed under ledges formed by tabular masses of limestone, and through some curious openings formed by the falling together of the summits of the rocks. At length we found ourselves beside our enemy of the morning. My guide halted for a minute or two scanning the torrent thoughtfully. I said that, as a guide, he ought to have a rope in such a place; but he retorted that, as no traveller had ever thought of coming there, he did not see the necessity of keeping a rope. He waded in. The struggle to keep himself erect was evident enough; he swayed, but recovered himself again and again. At length he slipped, gave way, did as I had done, threw himself flat in the water towards the bank, and was swept into the shallows. Standing in the stream near its edge, he stretched his arm towards me. I retained the pitchfork handle, for it had been useful among the boulders. By wading some way in, the staff could be made to reach him, and I proposed his seizing it. "If you are sure," he replied, "that, in case of giving way, you can maintain your grasp, then I will certainly hold you." I waded in, and stretched the staff to my companion. It was firmly grasped by both of us. Thus helped, though its onset was strong, I moved safely across the torrent. All danger ended here. We afterwards roamed sociably among the torrents and boulders below the Cave of the Winds. The rocks were covered with organic slime which could not have been walked over with bare feet, but the felt shoes effectually prevented slipping. We reached the cave and entered it, first by a wooden way carried over the boulders, and then along a narrow ledge to the point eaten deepest into the shale. When the wind is from the south, the falling water, I am told, can be seen tranquilly from this spot; but when we were there, a blinding hurricane of spray was whirled against us. On the evening of the same day, I went behind the water on the Canada side, which, I confess, struck me, after the experiences of the morning, as an imposture.

Still even this fall is exciting to some nerves. Its effect upon himself is thus vividly described by Mr. Bakewell, jun.:—"On turning a sharp angle of the rock, a sudden gust of wind met us, coming from the hollow between the falls and the rock, which drove the spray directly in our faces, with such force that in an instant we were wet through. When in the midst of this shower-bath the shock took away my breath; I turned back and scrambled over the loose stones to escape the conflict. The guide soon followed, and told me that I had passed the worst part. With that assurance I made a second attempt; but so wild and disordered was my imagination that when I had reached half way I could bear it no longer."*

To complete my knowledge it was necessary to see the fall from the river below it, and long negotiations were necessary to secure the means of doing so. The only boat fit for the undertaking had been laid up for the winter; but this difficulty, through the kind intervention of Mr. Townsend, was overcome. The main one was to secure oarsmen sufficiently strong and skilful to urge the boat where I wished it to be taken. The son of the owner of the boat, a finely-built young fellow, but only twenty, and therefore not sufficiently hardened, was willing to go; and up the river I was informed there lived another man who would do anything with the boat which strength and daring could accomplish. He came. His figure and expression of face certainly indicated extraordinary firmness and power. On Tuesday, the 5th of November, we started, each of us being clad in oilcloth. The elder oarsman at once assumed a tone of authority over his companion, and struck immediately in amid the breakers below the American Fall. He hugged the cross freshets instead of striking out into the smoother water. I asked him why he did so, and he replied that they were directed *outwards* not *downwards*. The struggle, however, to prevent the bow of the boat from being turned by them, was often very severe.

The spray was in general blinding, but at times it disappeared and yielded noble views of the fall. The edge of the cataract is crimped by indentations which exalt its beauty. Here and there, a little below the highest ledge, a secondary one jets out; the water strikes it and bursts from it in huge protuberant masses of foam and spray. We passed Goat Island, came to the Horseshoe, and worked for a time along the base of it, the boulders over which Conroy and myself had scrambled a few days previously lying between us and the base. A rock was before us, concealed and revealed at intervals, as the waves passed over it. Our leader tried to get above this rock, first on the outside of it. The water, however, was here in violent motion. The men struggled fiercely, the older one ringing out an incessant peal of command and exhortation to the younger. As we were just clearing the rock, the bow came obliquely to the surge; the boat was turned suddenly round and shot with astonishing rapidity down the river. The men returned to the charge, now trying to get up between the half-concealed rock and the boulders to the left. But the torrent set in strongly through this channel. The tugging was quick and violent, but we made little way. At length, seizing a rope, the principal oarsman made a desperate attempt to get upon one of the boulders, hoping to be able to drag the boat through the channel; but it bumped so violently against the rock, that the man flung himself back and relinquished the attempt.

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* 'Mag. of Nat. Hist.,' 1830, pp. 121, 122.

We returned along the base of the American Fall, running in and out among the currents which rushed from it laterally into the river. Seen from below the American Fall is certainly exquisitely beautiful, but it is a mere frill of adornment to its nobler neighbour the Horseshoe. At times we took to the river, from the centre of which the Horseshoe Fall appeared especially magnificent. A streak of cloud across the neck of Mont Blanc can double its apparent height, so here the green summit of the cataract shining above the smoke of spray appeared lifted to an extraordinary elevation. Had Hennepin and La Hontan seen the fall from this position, their estimates of the height would have been perfectly excusable.

From a point a little way below the American Fall, a ferry crosses the river in summer to the Canadian side. Below the ferry is a suspension bridge for carriages and foot-passengers, and a mile or two lower down is the railway suspension bridge. Between the ferry and the latter the river Niagara flows unruffled; but at the suspension bridge the bed steepens and the river quickens its motion. Lower down the gorge narrows and the rapidity and turbulence increase. At the place called the "Whirlpool Rapids" I estimated the width of the river at 300 feet, an estimate confirmed by the dwellers on the spot. When it is remembered that the drainage of nearly half a continent is compressed into this space, the impetuosity of the river's escape through this gorge may be imagined. Had it not been for Mr. Bierstädt, the distinguished photographer of Niagara, I should have quitted the place without seeing these rapids; for this, and for his agreeable company to the spot, I have to thank him. From the edge of the cliff above the rapids, we descended, a little I confess to a climber's disgust, in an "elevator," because the effects are best seen from the water level.

Two kinds of motion are here obviously active, a motion of translation and a motion of undulation—the race of the river through its gorge, and the great waves generated by its collision with, and rebound from, the obstacles in its way. In the middle of the river the rush and tossing are most violent; at all events, the impetuous force of the individual waves is here most strikingly displayed. Vast pyramidal heaps leap incessantly from the river, some of them with such energy as to jerk their summits into the air, where they hang suspended as bundles of liquid spherules. The sun shone for a few minutes. At times the wind coming up the river, searched and sifted the spray, carrying away the lighter drops, and leaving the heavier ones behind. Wafted in the proper direction, rainbows appeared and disappeared fitfully in the lighter mist. In other directions the common gleam of the sunshine from the waves and their shattered crests was exquisitely beautiful. The complexity of the action was still further illustrated by the fact, that in some cases, as if by the exercise of a local explosive force, the drops were shot radially from a particular centre, forming around it a kind of halo.

The first impression, and, indeed, the current explanation of these rapids is, that the central bed of the river is cumbered with large boulders, and that the jostling, tossing, and wild leaping of the water there are due to its impact against these obstacles. I doubt this explanation. At all events, there is another sufficient reason to be taken into account. Boulders derived from the adjacent cliffs visibly cumber the *sides* of the river. Against these the water rises and sinks rhythmically but violently, large waves being thus produced. On the generation of each wave there is an immediate compounding of the wave motion with the river motion. The ridges, which in still water would proceed in circular curves round the centre of disturbance, cross the river obliquely, and the result is that at the centre waves commingle which have really been generated at the sides. In the first instance we had a composition of wave motion with river motion; here we have the coalescence of waves with waves. Where crest and furrow cross each other, the motion is annulled; where furrow and furrow cross, the river is ploughed to a greater depth; and where crest and crest aid each other, we have that astonishing leap of the water which breaks the cohesion of the crests, and tosses them shattered into the air. From the water level the cause of the action is not so easily seen; but from the summit of the cliff the lateral generation of the waves and their propagation to the centre are perfectly obvious. If this explanation be correct, the phenomena observed at the Whirlpool Rapids form one of the grandest illustrations of the principle of *interference*. The Nile "cataract," Mr. Huxley informs me, offers examples of the same action.

At some distance below the Whirlpool Rapids we have the celebrated whirlpool itself. Here the river makes a sudden bend to the north-east, forming nearly a right angle with its previous direction. The water strikes the concave bank with great force, and scoops it incessantly away. A vast basin has been thus formed, in which the sweep of the river prolongs itself in gyratory currents. Bodies and trees which have come over the falls are stated to circulate here for days without finding the outlet. From various points of the cliffs above this is curiously hidden. The rush of the river into the whirlpool is obvious enough; and though you imagine the outlet must be visible, if one existed, you cannot find it. Turning, however, round the bend of the precipice to the north-east the outlet comes into view.

The Niagara season had ended; the chatter of sightseers had ceased, and the scene presented itself as one of holy seclusion and beauty. I went down to the river's edge, where the weird loneliness seemed to increase. The basin is enclosed by high and almost precipitous banks—covered, when I was there, with russet woods. A kind of mystery attaches itself to gyrating water, due perhaps to the fact that we are to some extent ignorant of the direction of its force. It is said that at certain points of the whirlpool pine-trees are sucked down, to be ejected mysteriously elsewhere. The water is of the brightest emerald green. The gorge through which it escapes is narrow, and the motion of the river swift though silent. The surface is steeply inclined, but it is perfectly unbroken. There are no lateral waves, no ripples with their breaking bubbles to raise a murmur; while the depth is here too great to allow the inequality of the bed to ruffle the surface. Nothing can be more beautiful than this sloping liquid mirror formed by the Niagara in sliding from the whirlpool.

The green colour is, I think, correctly accounted for in 'Hours of Exercise in the Alps.' In crossing the Atlantic I had frequent opportunities of testing the explanation there given. Looked properly down upon, there are portions of the ocean to which we should hardly ascribe a trace of blue; at the most a hint of indigo reaches the eye. The water, indeed, is practically *black*, and this is an indication both of its depth and its freedom from mechanically suspended matter. In small thicknesses water is sensibly transparent to all kinds of light; but as the thickness increases, the rays of low refrangibility are first absorbed, and after them the other rays. Where, therefore, the water is very deep and very pure, *all* the colours are absorbed, and such water ought to appear black, as no light is sent from its interior to the eye. The approximation of the Atlantic Ocean to this condition is an indication of its extreme purity.

Throw a white pebble into such water; as it sinks it becomes greener and greener, and, before it disappears, it reaches a vivid blue green. Break such a pebble into fragments, each of these will behave like the unbroken mass; grind the pebble to powder, every particle will yield its modicum of green; and if the particles be so fine as to remain suspended in the water, the scattered light will be a uniform green. Hence the greenness of shoal water. You go to bed with the black Atlantic around you. You rise in the morning and find it a vivid green; and you correctly infer that you are crossing the bank of Newfoundland. Such water is found charged with fine matter in a state of mechanical suspension. The light from the bottom may sometimes come into play, but it is not necessary. A storm can render the water muddy by rendering the particles too numerous and gross. Such a case occurred towards the close of my visit to Niagara. There had been rain and storm in the upper-lake regions, and the quantity of suspended matter brought down quite extinguished the fascinating green of the Horseshoe.

Nothing can be more superb than the green of the Atlantic waves when the circumstances are favourable to the exhibition of the colour. As long as a wave remains unbroken no colour appears; but when the foam just doubles over the crest like an Alpine snow-cornice, under the cornice we often see a display of the most exquisite green. It is metallic in its brilliancy. But the foam is necessary to its production. The foam is first illuminated, and it scatters the light in all directions; the light which passes through the higher portion of the wave alone reaches the eye, and gives to that portion its matchless colour. The folding of the wave, producing, as it does, a series of longitudinal protuberances and furrows which act like cylindrical lenses, introduces variations in the intensity of the light, and materially enhances its beauty.

We have now to consider the genesis and proximate destiny of the Falls of Niagara. We may open our way to this subject by a few preliminary remarks upon erosion. Time and intensity are the main factors of geologic change, and they are in a certain sense convertible. A feeble force acting through long periods, and an intense force acting through short ones, may produce approximately the same results. To Dr. Hooker I have been indebted for some samples of stones, the first examples of which were picked up by Mr. Hackworth on the shores of Lyell's Bay, near Wellington, in New Zealand, and described by Mr. Travers in the Transactions of the New Zealand Institute. Unacquainted with their origin, you would certainly ascribe their forms to human workmanship. They resemble flint knives and spear-heads, being apparently chiselled off into facets with as much attention to symmetry as if a tool guided by human intelligence had passed over them. But no human instrument has been brought to bear upon these stones. They have been wrought into their present shape by the windblown sand of Lyell's Bay. Two winds are dominant here, and they in succession urged the sand against opposite sides of the stone; every little particle of sand chipped away its infinitesimal bit of stone, and in the end sculptured these singular forms.*

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* "These stones, which have a strong resemblance to works of human art, occur in great abundance, and of various sizes, from half-an-inch to several inches in length. A large number were exhibited showing the various forms, which are those of wedges, knives, arrow-heads, &c., and all with sharp cutting edges.

"Mr. Travers explained that, notwithstanding their artificial appearance, these stones were formed by the cutting action of the wind-driven sand as it passed to and fro over an exposed boulder-bank. He gave a minute account of the manner in which the varieties of form are produced, and referred to the effect which the erosive action thus indicated would have on railway and other works executed on sandy tracts.

"Dr. Hector stated that although, as a group, the specimens on the table could not well be mistaken for artificial productions, still the forms are so peculiar, and the edges, in a few of them, so perfect, that if they were discovered associated with

The Sphynx of Egypt is nearly covered up by the sand of the desert. The neck of the Sphynx is partly cut across, not, as I am assured by Mr. Huxley, by ordinary weathering, but by the eroding action of the fine sand blown against it. In these cases nature furnishes us with hints which may be taken advantage of in art; and this action of sand has been recently turned to extraordinary account in the United States. When in Boston, I was taken by Mr. Josiah Quincey to see the action of the *sand-blast.* A kind of hopper containing fine silicious sand was connected with a reservoir of compressed air, the pressure being variable at pleasure. The hopper ended in a long-slit, from which the sand was blown. A plate of glass was placed beneath this slit, and caused to pass slowly under it; it came out perfectly depolished, with a bright opalescent glimmer, such as could only be produced by the most careful grinding. Every little particle of sand urged against the glass, having all its energy concentrated on the point of impact, formed there a little pit, the depolished surface consisting of innumerable hollows of this description. But this was not all. By protecting certain portions of the surface and exposing others, figures and tracery of any required form could be etched upon the glass. The figures of open iron-work could be thus copied; while wire gauze placed over the glass produced a reticulated pattern. But it required no such resisting substance as iron to shelter the glass. The patterns of the finest lace could be thus reproduced; the delicate filaments of the lace itself offering a sufficient protection. All these effects have been obtained with a simple model of the sand-blast devised for me by my assistant. A fraction of a minute suffices to etch upon glass a rich and beautiful lace pattern. Any yielding substance may be employed to protect the glass. By immediately diffusing the shock of the particle, such substances practically destroy the local erosive power. The hand can bear without inconvenience a sand-shower which would pulverize glass. Etchings executed on glass with suitable kinds of ink are accurately worked out by the sand-blast. In fact, within certain limits, the harder the surface, the greater is the concentration of the shock, and the more effectual is the erosion. It is not necessary that the sand should be the harder substance of the two; corundum, for example, is much harder than quartz; still, quartz-sand can not only depolish, but actually blow a hole through a plate of corundum. Nay, glass may be depolished by the impact of fine shot; the grains in this case bruising the glass before they have time to flatten and turn their energy into heat.

And here, in passing, we may tie together one or two apparently unrelated facts. Supposing you turn on, at the lower part of a house, a cock which is fed by a pipe from a cistern at the top of the house, the column of water, from the cistern downwards, is set in motion.

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human works, there is no doubt that they would have been referred to the so-called 'stone period.'"—*Extracted from the Minutes of the Wellington Philosophical Society, Feb.* 9, 1869.

By turning off the cock, this motion is stopped; and when the turning off is very sudden, the pipe, if not strong, may be burst by the internal impact of the water. By distributing the turning of the cock over half a second of time, the shock and danger of rupture may be entirely avoided. We have here an example of the concentration of energy in *time*. The sand-blast illustrates the concentration of energy in *space*. The action of flint and steel is an illustration of the same principle. The heat required to generate the spark is intense, and the mechanical action being moderate, must, to produce fire, be in the highest degree concentrated. This concentration is secured by the collision of hard substances. Calc-spar will not supply the place of flint, nor lead the place of steel in the production of fire by collision. With the softer substances, the *total* heat produced may be greater than with the hard ones, but to produce the spark, the heat must be intensely *localized*.

But we can go far beyond the mere depolishing of glass; indeed, I have already said that quartz-sand can wear a hole through corundum. This leads me to express my acknowledgments to General Tilghman,[*] who is the inventor of the sand-blast. To his spontaneous kindness I am indebted for some beautiful illustrations of his process. In one thick plate of glass a figure has been worked out to a depth of $\frac{3}{8}$ths of an inch. A second plate, $\frac{7}{8}$ths of an inch thick, is entirely perforated. Through a circular plate of marble, nearly half an inch thick, open work of the most intricate and elaborate description has been executed. It would probably take many days to perform this work by any ordinary process; with the sand-blast it was accomplished in an hour. So much for the strength of the blast; its delicacy is illustrated by a beautiful example of line engraving, etched on glass by means of the blast.[†]

This power of erosion, so strikingly displayed when sand is urged by air, renders us better able to conceive its action when urged by water. The erosive power of a river is vastly augmented by the solid matter carried along with it. Sand or pebbles caught in a river vortex can wear away the hardest rock; "potholes" and deep cylindrical shafts being thus produced. An extraordinary instance of this kind of erosion is to be seen in the Val Tournanche, above the village of this name. The gorge at Handeck has been thus cut out. Such waterfalls were once frequent in the valleys of Switzerland; for hardly any valley is without one or more transverse barriers of resisting material, over which the river flowing through the valley once fell as a cataract. Near Pontresina in the Engadin, there is such a case; the hard gneiss being now worn away to form a gorge through which the river from the Morteratsch glacier rushes. The barrier of the Kirchet above Meyringen is also a case in point. Behind it was a lake, derived from the glacier of the Aar, and over the barrier the lake poured its excess of water. Here the rock being limestone was in great part dissolved; but added to this we had the action of the sand particles carried along by the water, each of which, as it struck the rock, chipped it away like the particles of the sand-blast. Thus by solution and mechanical erosion the great chasm of the Fensteraarschlucht was formed. It is demonstrable that the water which flows at the bottoms of such deep fissures once flowed at the level of what is now their edges, and tumbled down the lower faces of the barriers. Almost every valley in Switzerland furnishes examples of this kind; the untenable hypothesis of earthquakes, once so readily resorted to in accounting for these gorges, being now for the most part abandoned. To produce the Cañons of Western America, no other cause is needed than the integration of effects individually infinitesimal.

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[*] The absorbent power, if I may use the phrase, exerted by the industrial arts in the United States, is forcibly illustrated by the rapid transfer of men like Mr. Tilghman from the life of the soldier to that of the civilian. General McClellan, now a civil engineer, whom I had the honour of frequently meeting in New York, is a most eminent example of the same kind. At the end of the war, indeed, a million and a half of men were thus drawn, in an astonishingly short time, from military to civil life. It is obvious that a nation with these tendencies can have no desire for war.

[†] The sand-blast will be in operation this year at the Kensington International Exhibition.

And now we come to Niagara. Soon after Europeans had taken possession of the country, the conviction appears to have arisen that the deep channel of the river Niagara below the falls had been excavated by the cataract. In Mr. Bakewell's 'Introduction to Geology,' the prevalence of this belief has been referred to: it is expressed thus by Professor Joseph Henry in the Transactions of the Albany Institute.* "In viewing the position of the falls, and the features of the country round, it is impossible not to be impressed with the idea that this great natural raceway has been formed by the continued action of the irresistible Niagara, and that the falls, beginning at Lewiston have, in the course of ages, worn back the rocky strata to their present site." The same view is advocated by Sir Charles Lyell, by Mr. Hall, by M. Agassiz, by Professor Ramsay, indeed, by most of those who have inspected the place.

A connected image of the origin and progress of the cataract is easily obtained. Walking northward from the village of Niagara Falls by the side of the river, we have to our left the deep and comparatively narrow gorge through which the Niagara flows. The bounding cliffs of this gorge are from 300 to 350 feet high. We reach the whirlpool, trend to the north-east, and after a little time gradually resume our northward course. Finally, at about seven miles from the present falls, we come to the edge of a declivity which informs us that we have been hitherto walking on table land. At some hundreds of feet below us is a comparatively level plain, which stretches to Lake Ontario. The declivity marks the end of the precipitous gorge of the Niagara. Here the river escapes from its steep mural boundaries, and in a widened bed pursues its way to the lake which finally receives its waters.

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* Quoted by Bakewell.

The fact that in historic times, even within the memory of man, the fall has sensibly receded, prompts the question, how far has this recession gone? At what point did the ledge which thus continually creeps backwards begin its retrograde course? To minds disciplined in such researches the answer has been and will be, at the precipitous declivity which crossed the Niagara from Lewiston on the American to Queenston on the Canadian side. Over this transverse barrier the united affluents of all the upper lakes once poured their waters, and here the work of erosion began. The dam, moreover, was demonstrably of sufficient height to cause the river above it to submerge Goat Island; and this would perfectly account for the finding by Sir Charles Lyell, Mr. Hall, and others, in the sand and gravel of the island, the same fluviatile shells as are now found in the Niagara River higher up. It would also account for those deposits along the sides of the river, the discovery of which enabled Lyell, Hall, and Ramsay, to reduce to demonstration the popular belief that the Niagara once flowed through a shallow valley.

The physics of the problem of excavation, which I made clear to my mind before quitting Niagara, are revealed by a close inspection of the present Horseshoe Fall. Here we see evidently that the greatest weight of water bends over the very apex of the Horseshoe. In a passage in his excellent chapter on Niagara Falls, Mr. Hall alludes to this fact. Here we have the most copious and the most violent whirling of the shattered liquid; here the most powerful eddies recoil against the shale. From this portion of the fall, indeed, the spray sometimes rises without solution of continuity to the region of clouds, becoming gradually more attenuated, and passing finally through the condition of true cloud into invisible vapour, which is sometimes reprecipitated higher up. All the phenomena point distinctly to the centre of the river as the place of greatest mechanical energy, and from the centre the vigour of the fall gradually dies away towards the sides. The Horseshoe form, with the concavity facing downwards, is an obvious and necessary consequence of this action. Right along the middle of the river the apex of the curve pushes its way backwards, cutting along the centre a deep and comparatively narrow groove, and draining the sides as it passes them.* Hence the remarkable discrepancy between the widths of the Niagara above and below the Horseshoe. All along its course, from Lewiston Heights to its present position, the form of the fall was probably that of a horseshoe; for this is merely the expression of the greater depth, and consequently greater excavating power of the centre of the river. The gorge, moreover, varies in width as the depth of the centre of the ancient river varied, being narrowest where that depth was greatest.

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* In the discourse the excavation of the centre and drainage of the sides action was illustrated by a model devised by my assistant, Mr. John Cottrell.

The vast comparative erosive energy of the Horseshoe Fall comes strikingly into view when it and the American Fall are compared together. The American branch of the upper river is cut at a right angle by the gorge of the Niagara. Here the Horseshoe Fall was the real excavator. It cut the rock and formed the precipice over which the American Fall tumbles. But since its formation, the erosive action of the American Fall has been almost nil, while the Horseshoe has cut its way for 500 yards across the end of Goat Island, and is now doubling back to excavate its channel parallel to the length of the island. This point I have just learned has not escaped the acute observation of Professor Ramsay.* The river bends; the Horseshoe immediately accommodates itself to the bending, and will follow implicitly the direction of the deepest water in the upper stream. The flexibility of the gorge, if I may use the term, is determined by the flexibility of the river channel above it. Were the Niagara above the fall sinuous, the gorge would obediently follow its sinuosities. Once suggested, no doubt geographers will be able to point out many examples of this action. The Zambesi is thought to present a great difficulty to the erosion theory, because of the sinuosity of the chasm below the Victoria Falls. But, assuming the basalt to be of tolerably uniform texture, had the river been examined before the formation of this sinuous channel, the present zigzag course of the gorge below the fall could, I am persuaded, have been predicted, while the sounding of the present river would enable us to predict the course to be pursued by the erosion in the future.

But not only has the Niagara River cut the gorge; it has carried away the chips of its own workshop. The shale being probably crumbled is easily carried away. But at the base of the fall we find the huge boulders already described, and by some means or other these are removed down the river. The ice which fills the gorge in winter, and which grapples with the boulders, has been regarded as the transporting agent. Probably it is so to some extent. But erosion acts without ceasing on the abutting points of the boulders, thus withdrawing their support and urging them gradually down the river. Solution also does its portion of the work. That solid matter is carried down is proved by the difference of depth between the Niagara River and Lake Ontario, where the river enters it. The depth falls from 72 feet to 20 feet, in consequence of the deposition of solid matter caused by the diminished motion of the river.†

In conclusion, we may say a word regarding the proximate future

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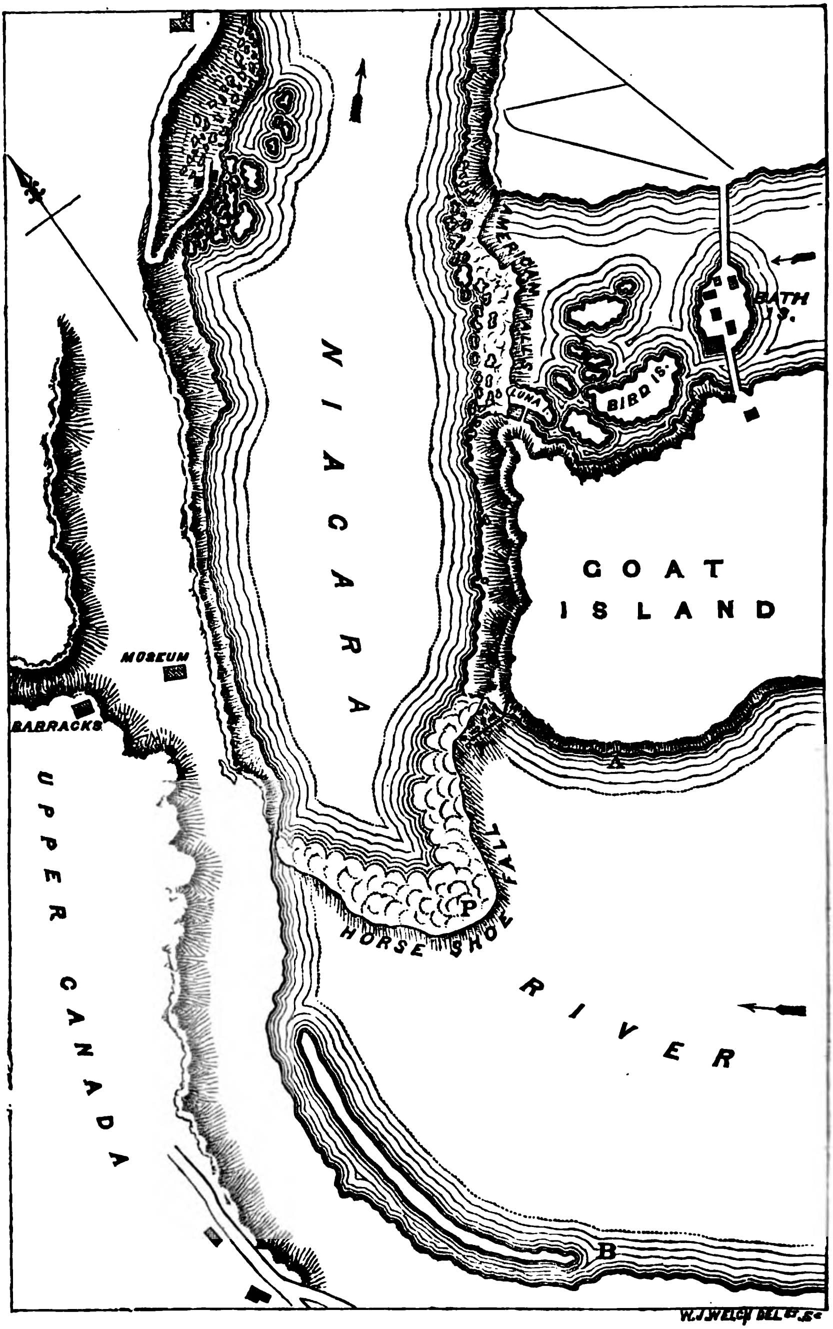
* His words are:—"Where the body of water is small in the American Fall, the edge has only receded a few yards (where most eroded) during the time that the Canadian Fall has receded from the north corner of Goat Island to the innermost curve of the Horseshoe Fall."—*Quarterly Journal of Geological Society, May,* 1859.

† Near the mouth of the gorge at Queenston, the depth, according to the Admiralty Chart, is 180 feet; well within the gorge it is 132 feet.



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of Niagara. At the date of excavation assigned to it by Sir Charles Lyell, namely, a foot a year, five thousand years or so will carry the Horseshoe Fall far higher than Goat Island. As the gorge recedes it will drain, as it has hitherto done, the banks right and left of it, thus leaving a nearly level terrace between Goat Island and the edge of the gorge. Higher up it will totally drain the American branch of the river; the channel of which in due time will become cultivable land. The American Fall will then be transformed into a dry precipice, forming a simple continuation of the cliffy boundary of the Niagara. At the place occupied by the fall at this moment we shall have the gorge enclosing a right-angle, a second whirlpool being the consequence of this. To those who visit Niagara a few millenniums hence I leave the verification of this prediction. All that can be said is, that if the causes now in action continue to act, it will prove itself literally true.

The accompanying highly instructive map has been reduced from one published in Mr. Hall's 'Geology of New York.' It is based on surveys executed in 1842, by Messrs. Gibson and Evershed. The ragged edge of the American Fall north of Goat Island marks the amount of erosion which it has been able to accomplish while the Horseshoe Fall was cutting its way southward across the end of Goat Island to its present position. The American Fall is 168 feet high, a precipice cut down, not by itself, but by the Horseshoe Fall. The latter in 1842 was 159 feet high, and, as shown by the map, is already turning eastward to excavate its gorge along the centre of the upper river. P is the apex of the Horseshoe, and T marks the site of the Terrapin Tower, with the promontory adjacent, round which I was conducted by Conroy. Probably since 1842 the Horseshoe has worked back beyond the position here assigned to it. Certainly the promontory at T appeared to me much sharper than it is here shown to be. In view of these considerations the foregoing prediction is merely the prospective statement of a fact requiring no great foresight to anticipate it.

[J. T.]

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## GENERAL MONTHLY MEETING,

Monday, April 7, 1873.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. President, in the Chair.

John Studdy Leigh, Esq. F.G.S. F.R.G.S.
Francis Sibson, M.D. F.R.S.
Mrs. Sibson.
Commander Edmund Hope Verney, R.N.

were *elected* Members of the Royal Institution.

With reference to the Resolution of the Managers, passed March 10 (see p. 57), the following letter from Dr. Bence Jones to the President and Managers was read:—

"84, Brook Street,
"*March* 11, 1873.

"Gentlemen,

"I shall indeed be glad, if hereafter by any words or deeds I can show my gratitude to you for the Resolutions you have passed regarding me.

"To receive your praises is most pleasant; but I should be happier if I could look forward with the hope of doing more to retain them. Your thanks are to me an all-sufficient and a complete reward for what I have done for the general good, and for the advancement of natural science at the Royal Institution.

"I value your praise far beyond any material reward, even though it takes the form of a bust, by Woolner, presented to me by the Members to prolong the remembrance of my connection with the home of Faraday and of Tyndall.

"But no words of mine can express my gratitude to you and to the Members; I can only beg you to believe me to be

"Your most grateful Servant,
"Henry Bence Jones."

The following letter from Dr. Warren De la Rue to the Treasurer was read:—

"The Observatory,
"Cranford, Middlesex, W.
"*March* 18, 1873.

"Dear Mr. Spottiswoode,

"I feel very great pleasure in contributing to the Dr. Bence Jones Testimonial Fund, and enclose a cheque for the amount at which the maximum contribution has been limited. I venture also to send a Donation [100*l.*] towards the equipment of the new Laboratories, as a further mark of my high appreciation of the eminent services which Dr. Bence Jones has rendered to the Royal Institution during the long period he has held the onerous position of Honorary Secretary. We all know that the erection of new Laboratories, better adapted to promote the advancement of science, was to Dr. Bence Jones a matter of anxious solicitude for some years past, and it will always be remembered that the completion of the scheme was the crowning act of his enlightened career in connection with the Institution.

"I am,
"Yours very truly,
"Warren De la Rue."

Resolved—That the special thanks of the Members be returned to Dr. De la Rue for his liberal Donation.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

**FROM**

*Académie des Sciences de l'Institut de France*—Mémoires par divers Savants. Tome XX. 4to. 1872.

*Actuaries, Institute of*—Journal, No. 90. 8vo. 1873.

*Agricultural Society of England, Royal*—Journal, No. 17. 8vo. 1873.

*Alcock, Col. M.R.I. (the Author)*—The Relative Power of Nations. (K 99) 8vo. 1872.

*Antiquaries, Society of*—Proceedings, Vol. V. No. 5. 8vo. 1872.

*Asiatic Society of Bengal*—Journal, Nos. 177, 178.
Proceedings, 1872, No. 9. 8vo.

*Astronomical Society, Royal*—Proceedings, Vol. XXXIII. No. 4. 8vo. 1873.

*Belgique, Académie Royale de*—Bulletins, 2e Série, Tomes XXXI.–XXXIV. 8vo. 1871-72.
Centième Anniversaire de Fondation (1772–1872). 2 vols. 8vo. 1872.
Annuaire, 1872–73. 16to.

*British Architects, Royal Institute of*—Sessional Papers, 1872–73. Nos. 7, 8. 4to.

*British Museum Trustees*—Catalogue of Hemiptera Heteroptera, Part 6. 8vo. 1873.

*Chemical Society*—Journal for March, 1873. 8vo.

*Dublin Society, Royal*—Journal, Vol. VI. No. 2. 8vo. 1872.

*Editors*—American Journal of Science for March, 1873. 8vo.
Athenæum for March, 1873. 4to.
Chemical News for March, 1873. 4to.
Engineer for March, 1873.
Food Journal for March, 1873. 4to.
Journal for Applied Science for March, 1873. Folio.
Journal of Gas-Lighting for March, 1873. 4to.
Nature for March, 1873. 4to.
Nautical Magazine for March, 1873. 8vo.
Once a Week for March, 1873. 8vo.
Pharmaceutical Journal for March, 1873, 8vo.
Telegraphic Journal for March, 1873.

*Franklin Institute*—Journal, No. 566. 8vo. 1873.

*Geographical Society, Royal*—Proceedings, Vol. XVI. No. 5.; Vol. XVII. No. 1. 8vo. 1873.

*Geological Institute, Imperial, Vienna*—Jahrbuch, 1872. No. 4. 8vo.
Verhandlungen, 1872. Nos. 14–17.
General Register: Jahrbücher und Verhandlungen, 1860–70. 4to. 1872.

*Gresham Committee*—Catalogue of Books, Music, Pictures, &c., in the Library of Gresham College. 8vo. 1872.

*Linnean Society*—Journal, No. 69. 8vo. 1873.

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## WEEKLY EVENING MEETING,

Friday, April 25, 1873.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

PROFESSOR W. H. FLOWER, F.R.S.

### *On Palæontological Evidence of Gradual Modification of Animal Forms.*

I NEED scarcely say that one of the greatest, if not absolutely the greatest problem which has ever exercised the minds of naturalists is that of the fixity or the mutability of species.

Are the various specific forms under which animal and vegetable life exist upon earth, now and in all times past, fixed within certain narrow limits of variation, and did each originally appear upon the earth without genetic connection with any previously existing forms, having been created *de novo* in fact? or have these different species been produced by gradual modification from pre-existing living forms, under the influence of certain laws, at present very imperfectly understood, acting through vast and indefinite periods of time?

It is clear that these two views are strongly opposed to each other. Both have been held and still are held by men who are justly considered masters in the branch of knowledge to which they relate; and the solution of the question will exercise so important an influence on the progress of zoology that any real contribution towards it should be one of the most welcome additions to science that a naturalist of the present day can make.

The question is, indeed, so far-reaching, so all-pervading, that it meets us everywhere in the study of every group of animal or vegetable life, and in almost every aspect in which the study can be carried out.

It bears largely upon, and is greatly illustrated by, descriptive zoology or botany. It adds vastly to the interest of the pursuit of anatomy, by calling out the meaning of rudimentary structures and so-called typical resemblances; it elucidates obscure questions relating to the habits and instincts of animals; it brings into prominence the signification of various facts of geographical distribution, and the life it throws into the study of palæontology is too obvious to need remark.

Evidences bearing upon, either for or against, the theory of *evolution* or *descent* can be collected from all these sources. I need only refer to Mr. Darwin's works, which must be familiar to you all, in illustration of the great variety and number of the branches of science which can be brought to throw light upon it. Indeed, in a subject like this, where direct observation can count for little, in consequence of the extreme shortness of the observing time of any individual compared with the enormous period required for the assumed changes, it is only by the accumulation of a vast number of facts from various sources, and observing the direction in which they all point, that anything like proof can be obtained.

Leaving aside, for the present occasion, all other sources of evidence in favour of either of these views, I propose this evening to enter only upon one which is in some respects, as all must admit, the most important, as it comes nearer than any other to show what actually has been the history of our existing species in times past; for as the most natural and conclusive way of ascertaining the method by which a nation has arrived at its present condition of society, customs, laws, &c., would certainly be to examine into the preserved records of its past history, so it must be with the present condition of animal and vegetable life.

We all know that such records have been preserved, that the solid rocks beneath our feet in many places teem with the actual remains of creatures which lived and died, thousands or millions of years ago.

Why should they not yield to us the knowledge we are all so eager to acquire?

If species are and ever have been immutable, shall we not find the same hard and fast lines surrounding each as we do now? Shall we not find long series of similar forms following without change on an abrupt commencement? If the other alternative be correct, ought we not to find specimens of all the various stages through which the wonderful variety we meet with now has been brought about? Every gap which now so widely separates group from group ought to be filled up, and the various phases of modification should follow through the successive eras of geological time.

Now, there can be no hesitation in saying that the evidence of palæontology, in the present state of the science, does not reveal the last-described condition of things. Notwithstanding the vast increase of our knowledge in recent years, very many large groups of animals stand completely isolated, and the more nearly allied forms are mostly separated from each other by tolerably definite intervals.

Is, then, the question decisively answered against evolution or derivation by palæontology?

We must pause before we can join in the assertion that it is. The subject is far more complex than it may seem at first.

Before going further, a proper estimate must be arrived at of the nature and value of our evidence, and in doing this we must give full weight to the considerations derived from the "imperfection of the geological record" so strikingly elucidated in Mr. Darwin's chapter on the subject.

To those who have not fully considered this question, it is difficult to conceive how immense is the interval between our excessively fragmentary knowledge of extinct animals, and that perfect palæontological record which would imply evidence, first, of every form of life that has ever existed, and, secondly, of the period at which it existed.

If there were time, I might dwell long upon this part of the subject, but I must leave you to imagine, I. What the chances are against the fossilization of any animal that dies. II. What the chances of the stratum in which some fossil remains have been embedded being itself preserved during the constant changes going on on the earth's surface, and ultimately appearing in a situation accessible to man's research. III. What the further chances against their being so found, even if they should have been preserved in an accessible locality.

I might refer you to the exceedingly minute portion of the earth's surface which has yet been really explored palæontologically; to the cases that are occurring every day of new and most unexpected forms and of whole species or orders, known only by an isolated individual, as the *Archæopteryx* of the Solenhofen oolite; to say nothing of more recondite speculations in the work above referred to, on the improbability of preservation of intermediate forms, owing to variation having usually been most rife during periods of elevation, when fossilization is less likely to occur.

All these show in such a striking manner the extremely little value of negative evidence in palæontology, that I am quite justified in asking you never for a moment to leave it out of consideration in thinking of, or reasoning on, what is to follow.

Such being the material with which we have to deal, it will be seen that we must go to work upon it in a most careful and circumspect manner. We cannot rush at conclusions, but must be content cautiously, and often with much labour and anxiety, to piece together our facts, scrupulously observing the minutest hints, and following out the direction indicated by often very obscure signs, before we can reconstruct even an outline of the fabric from which we hope to gain an idea of the past history of the beings of which we treat.

I have selected for illustration of the subject this evening the division or order of Mammals called by naturalists UNGULATA, or hoofed animals, chiefly because it is the one of which the palæontological history—at least in the tertiary period (for beyond that we cannot trace it)—is better known than any other, and as that of which the classification,—that is, the relations of its various sub-groups to each other,—is on the whole better understood than in most other zoological divisions.

The order includes the most familiar of our domestic animals, and with the general appearance of the rest we are most of us well acquainted, thanks to the Zoological Gardens. They are the various forms of horses, asses, and zebras, the rhinoceroses and the tapirs, the pigs, hippopotamus, camels, deer, antelopes, sheep, oxen, and goats.

They are essentially herbivorous (though some few may be more or less omnivorous), and their teeth are modified accordingly. Their limbs are adapted for carrying the body in ordinary terrestrial progression, and are of very little use for any other purpose, such as climbing, seizing prey, or carrying food to the mouth. They never have clavicles or collar-bones, and their toes never exceed four in number (the digit which corresponds to the first of the complete pentadactyle foot being always wanting), and have the ends encased in hoofs instead of nails or claws. The species at present existing are very numerous, and widely diffused over the earth's surface, being wanting only in the Australian province. These Ungulate animals are divided into two natural groups, each having very many characters in common, the establishment of which, though contrary to the views of the great naturalists at the beginning of this century, has been a great gain to zoological science, especially as this division pervades all the known extinct as well as recent forms; and although some forms of either group may present some partial approximation to the other, no directly intermediate species are known. It is important, therefore, to apprehend thoroughly the distinction between these groups, which have received from Professor Owen the names of *Perissodactyle*, or odd-toed, and *Artiodactyle*, or even-toed, from one of their most striking external characteristics. The first have the toes of both feet arranged symmetrically to a line drawn through the middle of what would be the third toe of the typical pentadactyle foot, which toe is always the largest, and in some cases the only one fully developed. In the second, the toes are arranged symmetrically to a line drawn between the third and fourth toes, so that these two toes are equally developed, and may be alone present, or may be supplemented by an outer pair (the second and fifth), often in a more or less rudimentary condition. Besides these distinctions in the limbs, there are so many others correlated with them in the number of the vertebræ, the structure of the cranial bones, of the teeth, of the digestive organs, &c., that there can be no question about their forming natural divisions, very important to palæontologists, as it often happens that the position of an extinct and little-known form can be determined from a very small fragment of bone.

Each of these groups is further divided into genera, the names of which in what appears, in the present state of knowledge, to be their natural position and relation to each other, are indicated on the diagram. From this, it will be seen, that the existing Perissodactyles (excluding Hyrax, the position of which is doubtful, though I am inclined to consider it as an aberrant member of this group) consist of three groups, the tapirs, the rhinoceroses, and the horses, each represented by but few species, and (except in the case of the horse, through the agency of man) of rather restricted geographical distribution.

These groups at present are separated by very decided intervals, so much so, that one of them, containing the horses, has been considered by many naturalists as forming an order apart, the *Solidungula*.

The existing Artiodactyles range themselves around two principal types, the tubercular-toothed, or bunodont, and the crescentic-toothed, or selenodont.* To the former belong the pigs in all their modifications, including the babirussa and wart-hogs, and the hippopotamus and peccary. To the latter the ruminants, *i. e.* vast numbers of species of animals included under the general designation of sheep, oxen, goats, antelopes, deer, musks, giraffes, and the two allied though aberrant forms, the camels, and the *Tragulidæ* or chevrotains, an interesting little group long confounded with the musk deer.

The two extremes of this division, represented by the pigs and the hollow-horned ruminants, seem to have very little in common at first sight, and if we were acquainted with the organization only of the existing species, we might be justified in treating them as belonging to very distinct groups. But even among existing forms there are some examples, which may almost be called intercalary types, so widely do they depart from the group to which they are most nearly relative in the direction of the other.

These are among the bunodonts, the little South American peccaries (*Dicotyles*), and among the selenodonts, in a far greater degree, the chevrotains (*Tragulus*). The latter in many remarkable characters deviate strongly from the ruminants, and approach the pigs, or rather, as will be shown presently, to the generalized type of the entire group.

Such being the present condition of the order, what does palæontology reveal of its past history?

In the first place, it is most necessary to bear in mind the provisional character of all classifications of extinct animals, because of our imperfect knowledge of their structure; but endeavouring to

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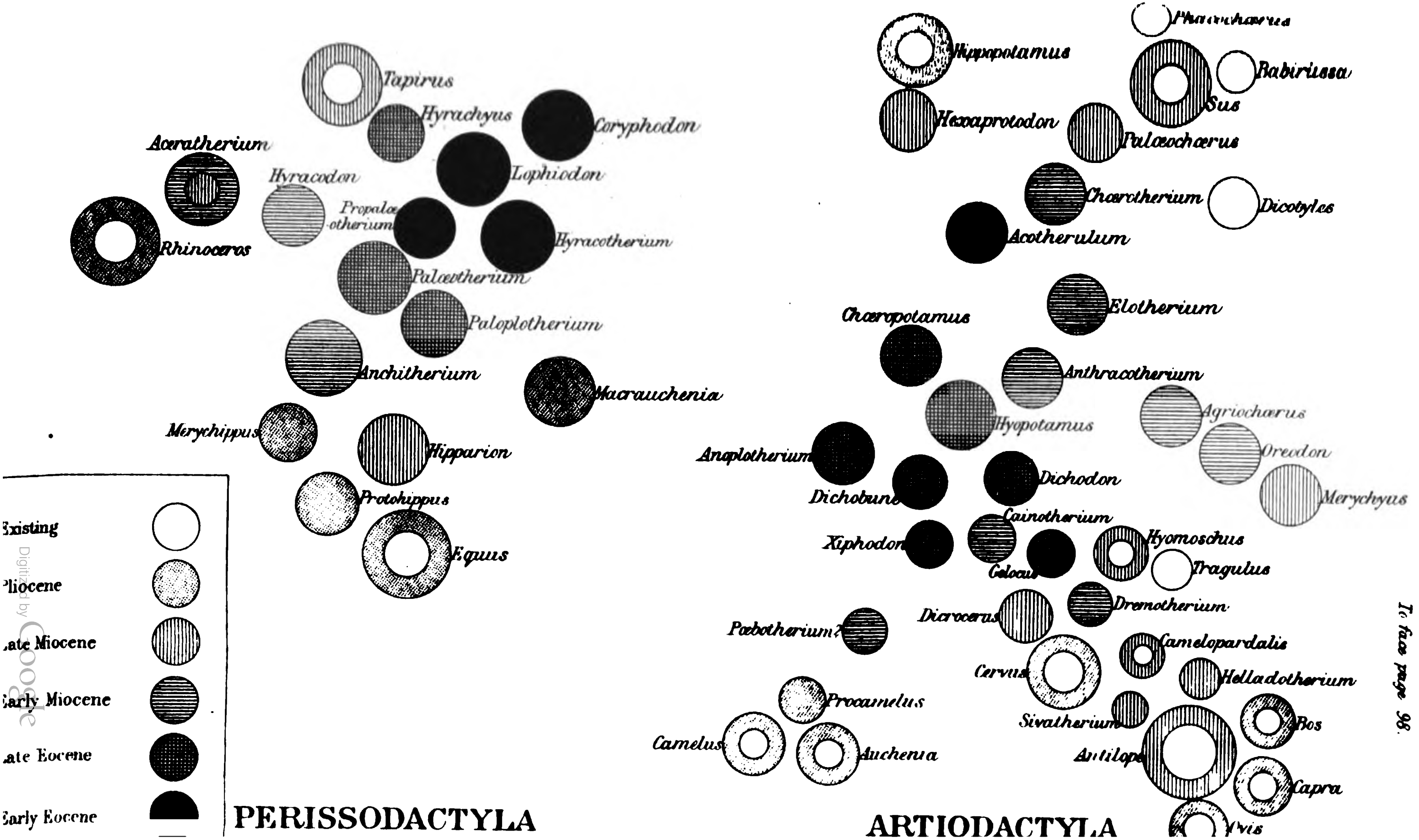
* I borrow these convenient terms from a paper lately read before the Royal Society by Dr. W. Kowalevsky.

# PROVISIONAL CLASSIFICATION OF UNGULATA.



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make the best use of what little we possess, I have added in the diagram all the best known extinct forms somewhere near the position, in relation to the existing forms and each other, in which their affinities would place them; and by the different colours their relation as regards time is shown.

The tertiary period, with which we are now alone concerned, has here been divided for convenience into six epochs. Of course, it were possible to have gone into minute details and made many more divisions, but it would have made the diagram less clear, and it is best, perhaps, not to attempt to refine too much in this somewhat tentative exposition of a biological history, especially as there is still much uncertainty as to the exact relative age of many of our fossiliferous strata.

The epochs chosen are the recent (including the pleistocene), the pliocene, late and early miocene, and late and early eocene, each represented by a different colour. It is not meant that if a genus or group is here assigned to one of these epochs, that some of its members may not have extended in some degree beyond its limits (as it must be always remembered that the boundaries of these epochs are quite artificial), either before or after, but that the period assigned to it was that in which it most chiefly flourished. When two colours are represented, one within another, it signifies that the group existed in both, and of course in all intervening periods.

To begin with the Perissodactyles. The earliest known forms constitute a family called *Lophiodontidæ*, composed of the genera *Lophiodon*, *Coryphodon*, and *Hyracotherium*. Of these animals little is known except the teeth, which however indicate rather a primitive or root form, from which, by modification, all the other teeth of Perissodactyles can be derived. The elevations and depressions of the molar teeth of *Lophiodon*, for instance, are arranged on a pattern which is the best key to that of all others of the sub-order; and it is by going back, as it were, to it, that we can understand and compare all the other variously modified, and often more complicated, forms. Moreover, these Lophiodonts possess a dental character which distinguishes them from all other *Perissodactyles*, and brings them into a more generalized ungulate type, for which reason I place them nearest to the earlier forms of artiodactyles—that is, that all the premolars are smaller and of a simpler form than the true molars. Whether they possessed any modification of the limbs or other structures which bear them out in this position, we unfortunately cannot say.

At a somewhat later epoch in the earth's history appeared on the scene the *Palæotheriidæ*, an important group, containing animals the osseous structure and dentition of which are completely known, chiefly through the famous researches of Cuvier into the fossils found in the gypsum quarries at Montmartre. These were animals something like existing tapirs, with three toes on each foot, complete and distinct radius and ulna and tibia and fibula, complete typical number of teeth, *i.e.* $i\,\frac{3}{3}\;c\,\frac{1}{1}\;p\,\frac{4}{4}\;m\,\frac{3}{3} = 44$; but the molar teeth modified in pattern from that of the Lophiodonts. They flourished in the later eocene, after which period they are no longer met with. They have been divided into several genera, but Gaudry has shown that these are united by transitional forms, and present a gradual series of modifications, corresponding with successive geological epochs.* Another offset, from the ancient Lophiodont stock (with which it appears to be connected through the American eocene *Hyracahyus*), constitutes the family *Tapiridæ*, first known in the miocene and continued with scarcely any modification to our own day, and therefore a most interesting form to contemplate in its living state, as it brings back, in the most striking way, the general facies of the fauna of those ancient times. In one respect the tapir is remarkable among Perissodactyles, as it has on its fore feet as many as four toes, thus retaining a primitive or generalized character. The other two existing forms, the rhinoceros and the horse, appear to be more direct modifications of the Palæotherium type, though in different directions. The existing rhinoceros closely resembles the Palæotherium in the general structure of its skeleton, limbs, number of toes, &c., and in the general pattern of the molar teeth; it differs, however, in the greatly reduced number of front teeth, incisors and canines, which in the African two-horned species are often absolutely wanting; and also in the possession of those singular epidermal appendages to the face, the well-known horns, either one or two in number. Now palæontology points out with tolerable precision the intermediate steps by which these modifications have been brought about. A small ancient rhinoceros has been found in the early miocene of North America, to which Leidy has given the name of *Hyracodon*, which had no horn, and had the complete number of incisor and canine teeth, and was in many ways, at least as far as the skull and teeth are concerned, intermediate between *Palæotherium* and *Rhinoceros* proper. The earlier known European rhinoceroses have had the name *Aceratherium* given to them, the small size of the nasal bones being apparently quite unfitted to support such a weapon as a horn. The resemblance of their skull to *Palæotherium* has been pointed out by H. v. Meyer.

The more recent fossil rhinoceroses present wonderfully intermediate forms between some of the existing species, as *R. pachygnathus*, of Pikermi, as Gaudry has shown, is about equally related to the two species of modern African rhinoceros, and might have been (upon the derivative hypothesis) the ancestor of both. In the same way the Himalayan *R. sivalensis* appears to be related to the modern *R. indicus* and *sondiacus*, and the *R. schleirmacheri* to the Asiatic two-horned species. One special line of variation indicated chiefly by the ossification of the nasal septum culminated in the *R. tichorhinus*, which became extinct only in the most recent geological epoch. The history of this small group alone in its bearings upon evolution, might occupy many lectures; I must content myself now only with one observation, borrowed from Mr. Boyd Dawkins, that in all modern rhinoceroses the molar teeth have deeper crowns than in those which existed prior to a certain epoch, so that the height of these teeth alone will serve to distinguish a pleistocene from a pliocene form, in other respects closely allied. The value of this observation will be illustrated in the sequel.

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* 'Remarques sur les Paloplotherium,' Nouv. Archives du Muséum d'Histoire naturelle, tom. i., 1865, p. 15.

The next line of modification from *Palæotherium*, is that which culminates in the most specialized of mammals, the modern horse, an animal we are so accustomed to look at that we scarcely ever notice the most remarkably adaptive character of its structure for its special mode of life. If we were not acquainted with the horse (and here of course I include its immediate allies, the asses and zebras,) we could scarcely conceive of an animal whose only support was the tip of a single toe on each extremity, to say nothing of the singular conformation of its teeth and other organs. So striking have these characters appeared to many zoologists, that the animals possessing them have been reckoned as an order apart called *Solidungula;* but palæontology has revealed that in the structure of its skull, its teeth, its limbs, the horse is nothing more than a modified *Palæotherium;* and though still with gaps in certain places, many of the intermediate stages of these modifications are already known to us, being the *Paloplotherium*, *Anchitherium*, *Merychippus*, and *Hipparion*. On this very interesting point, which looks more like a real genealogical history than any other known, however, I need not dwell, as it was so fully treated of in a lecture delivered in this theatre three years ago by Professor Huxley—a lecture entitled the "Pedigree of the Horse."

Lastly, there is *Macrauchenia*, a curiously modified Perissodactyle found in pleistocene times in South America, apparently another derivative of the palæotherium type, presenting resemblance (though perhaps only analogical) to some of the artiodactyles, especially the camels.

Directly intermediate forms between *Macrauchenia* and the other animals of its group are not yet known; but considering how little evidence we have of the animal life of the middle or older tertiaries of South America, this is not to be wondered at.

On the whole it will be seen, that taking actual anatomical characters alone, palæontological research, even so far as it has yet been carried, bridges over most of the gaps existing between the modern form of Perissodactyles, entirely abolishing for instance the order *Solipedia*, as it is impossible to draw a satisfactory line where the animal ceases to be equine and becomes a palæotheroid; some drawing it between *Anchitherium* and *Palæotherium*, some between *Anchitherium* and *Hipparion*. Moreover, and this is most important, the lines from the modern more specialized forms converge towards the ancient more generalized forms; so that if we could get a side-view of what is shown in the diagram, the earliest forms at the bottom and the latest at the top, we should have lines (broken it is true, here and there) diverging from a common, or near a common centre, towards a circumference above—a view, in fact, of the conventional genealogical tree.

We turn now to the *Artiodactyles*, represented at present by the scattered groups before spoken of, clustering round two type forms so widely sundered in their structure and habits as the pig and the ox; but the former history of this division yields a totally different state of things. Of early eocene Artiodactyles we know very little at present; but in the later divisions of the same epoch forms appeared, such as *Anoplotherium*, *Dichobune*, *Chœropotamus*, and *Hyopotamus*, which were certainly neither pigs nor ruminants, but which partook remarkably of the characters of both. They had the complete number of teeth, *i.e.* incisors and canines, like modern pigs, but molars with indications of the crescentic pattern so characteristic of ruminants. They had two or four toes; but the metacarpals and metatarsals were not united to form a cannon bone as in ruminants, and they wanted the horny appendages to the head, so usually met with in the modern representatives of that group. From some of these central forms, or more probably from a still earlier allied group indicated by the genus *Acotherulum*, or by some other still undescribed remains from Mauremont, transitions can be traced with few breaks, through the successively modified miocene genera *Chœrotherium* and *Palæochœrus* to the genus *Sus*, or true pig, in which the dentition undergoes some remarkable specializations, as the upturning of the upper canines, and great development and extremely tuberculated character of the posterior molars, which are both singularly exaggerated in some modern offsets of the pig family, the first in the babirussa, and the second in the wart-hog (*Phacochœrus*). More distantly related to the true pigs are the hippopotamus on the one hand, and the peccary on the other. In relation to the first, not found anteriorly to the latest miocene, it is significant that the earliest known forms had the more generalized number of incisor teeth (six) instead of four as in the modern hippopotamus, and hence has been made into a genus by itself, called *Hexaprotodon*.

The researches of Leidy into the ancient (miocene and early pliocene) fauna of Nebraska have furnished evidence of a remarkable group of animals now entirely extinct, the *Oreodontidæ*, the characters of which are perfectly intermediate between those of the pigs and the ruminants; animals with pig-like feet and complete number of incisors, canines, and molars, but with the latter important set of teeth, formed precisely on the same type as those of the deer. Within this particular group Leidy has noted a curious series of slight modifications coinciding with the successive age of the strata in which the remains were found. *Agriochœrus*, the most ancient, approaches nearer to *Chœropotamus*, has orbits open behind and very shallow-crowned teeth. Then follows *Oreodon* proper, and lastly *Merychyus*, more like the modern ruminants.*

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* 'The Extinct Mammalian Fauna of Dakota and Nebraska,' 1869.

To return to the European forms, in the genus *Gelocus*, where the union of the two principal bones of the metapodium first occurs, Kowalevsky has noticed the gradual way in which this change seems to have been brought about in successive epochs of eocene and early miocene strata, at first free in the young, and only coalescing in old animals, afterwards coalescing at a much earlier age. The gradual perfecting of the foot by the development of the ridge round the lower articular end of the metapodium in later forms, the ridge being quite wanting in early forms of the same group, has been noticed by the same author in many different series of Ungulates.*

During the miocene period the peculiar dental characteristics of the modern ruminants, especially the loss of the upper incisors, were developed, all selenodont artiodactyles henceforth showing it. Of this early race of imperfect ruminants, still retaining many generalized characters, especially in the skull, the cervical vertebræ, fibula, stomach, &c., the chevrotains (*Tragulidæ*) are the survivors, especially the West African *Hyomoschus*, which has existed almost unchanged since the late miocene of Sansans and Steinheim. Then for the first time the appendages called antlers were introduced, but only in a comparatively rudimentary condition, with long pedicles and few branches, as in the modern Muntjaks. It was not till pliocene and especially pleistocene epochs that the wonderful and luxuriant variety of cervine antlers reached their full development. As offsets of the deer group, the giraffe, the gigantic Siwalik *Sivatherium*, and the *Helladotherium* of Greece may be mentioned, the two latter having become extinct, apparently without descendants.

Later still, the yet more specialized forms of hollow-horned ruminants appear—forms which now dominate the earth, being of all Ungulates the most widely diffused and most numerous in species, in individuals, and in outward variety, though in essential structure all alike. One of their principal characteristics is the modification of their molar teeth in the same way as in the modern horses, to which in some respects they seem to form a parallel group. The difference between the molar tooth of a hollow-horned ruminant and that of a deer consists in the great lengthening of the crown without any change in the pattern of the enamel folds, and in the addition of cement to support these folds. This alteration did not take place suddenly, and the crowns of teeth of the artiodactyles before the time of deer were still shorter than in those animals.

Among the deer themselves, as Lartet observed,† the most ancient have very short-crowned molars, and the depressions on the surface are so shallow that the bottom is always visible, while in the Cervidæ of the more recent tertiary periods, and especially the pleistocene and living species, these same cavities are so deep that, whatever be the state of dentition, the bottom cannot be seen. This

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* See also his interesting observations on adaptive and inadaptive modifications of the feet.—*Proc. Roy. Soc., Feb. 6th*, 1873.

† 'Comptes Rendus,' 1868, tom. 66, p. 1119.

(he says) is a perfectly reliable rule for distinguishing the ancient from the more modern forms of deer, and can be applied to other animals as well as the Cervidæ. From it he surmises that the duration of the life of modern is greater than that of ancient deer. The same careful observer also remarks that a gradual progress is observed in the volume of the brain and complexity of its surface, as deduced from casts of the interior of the skull, from which fact he concludes that a gradual growth of vital energy and intelligence has occurred as the effect of the tendency of animated nature towards improvement, of which the cause is always acting, and the limits indefinite.

Thus the history of the Even-toed Ungulates tells the same story as that of the Perissodactyles. The modern forms are placed along lines which converge towards a common centre. Moreover, the lines of both groups, to a certain extent, approximate; but within the limits of our knowledge they do not meet. Both artiodactyles and perissodactyles existed low down in the eocene, just as Carnivores, Insectivores, bats, rodents, and other great groups then existed with boundary lines as distinctly marked as now.

Was the order, according to which the introduction of new forms seems to have taken place since that epoch, then entirely changed? or did it continue as far back as the period when these lines would have been gradually fused into a common centre?

Here we are landed in the region of pure speculation; but bolder travellers than I have endeavoured to penetrate its mysteries, as may be seen by a perusal of Professor Huxley's presidential address to the Geological Society for 1870.*

I have so far confined myself within the region of the known, and shown that at least in one group of animals the facts which we have as yet acquired point to the former existence of various intermediate forms, so numerous that they go far to discredit the view of the sudden introduction of new species.

They show also many cases of gradual modification of particular organs, probably always to the benefit of the race, and also a general progress from lower to higher or more specialized types; though, as in all other cases of progress (human civilization, for instance), attended with many exceptions, some local and temporary, some only apparent.

Whether the inferences which seem to me to follow from these facts are true or not, may still be an open question; for the sake of the stimulus that an open question of this sort lends to scientific research I am very glad that it is so; but if true, if we are led by them to the conclusion that the world we live in is a world of gradual growth and progress, and orderly evolution, what grander view of the Creation and the history of that world can we have opened to us?

[W. H. F.]

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* Republished in his 'Critiques and Addresses,' 1873.

## ANNUAL MEETING,

Thursday, May 1, 1873.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

The Annual Report of the Committee of Visitors for the year 1872 was read and adopted.

Fifty-eight new Members were elected in 1872.

Sixty-three Lectures and Nineteen Evening Discourses were delivered during the year 1872.

The Books and Pamphlets presented in 1872 amounted to 95 volumes, making, with those purchased by the Managers, a total of 241 volumes added to the Library in the year, exclusive of periodicals.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to the Professors, for their services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

**President**—Sir Henry Holland, Bart. M.D. D.C.L. F.R.S.
**Treasurer**—George Busk, Esq. F.R.C.S. F.R.S.
**Secretary**—William Spottiswoode, Esq. LL.D. M.A. F.R.S.

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George Berkley, Esq. C.E.
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Robert P. Roupell, Esq. M.A. Q.C.
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The Hon. John William Strutt.
Sir Charles Wheatstone, D.C.L. F.R.S.
Colonel Philip James Yorke, F.R.S.

### Visitors.

Andrew Whyte Barclay, M.D.
Hon. Robert Bourke, M.P.
John Charles Burgoyne, Esq.
Alexander John Ellis, Esq. F.R.S.
George Edward Eyre, Esq. M.A.
Thomas Williams Helps, Esq. M.A.
Alfred Gutteres Henriques, Esq. F.G.S.
Thomas Hyde Hills, Esq.
Robert Pilkington Linton, Esq. F.R.C.S.
William Millar, Esq.
Edward Henry Moscrop, Esq.
A. C. Brisbane Neill, M.D.
The Lord Josceline W. Percy.
William Salmon, Esq.
Henry Vaughan, Esq.

## WEEKLY EVENING MEETING,

Friday, May 2, 1873.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

Dr. J. Emerson Reynolds,
PROFESSOR OF CHEMISTRY, ROYAL DUBLIN SOCIETY, AND KEEPER OF THE MINERAL DEPARTMENT,

## *On Alcohols from Flint and Quartz.*

In appearing before you this evening to lecture on "Alcohols from Flint and Quartz," you will permit me at the outset to explain, and even materially to extend, the title of my discourse.

I do not propose to show that spirits can be extracted from flint and quartz by mechanical processes; but I hope to satisfy you that by indirect and purely chemical means we can obtain from these familiar and widely-diffused minerals, and from native silicates, bodies resembling in chemical action, and even in appearance, the well-known alcohol of wine.

Carbon has hitherto been considered the sole alcohol-forming element; but we shall see that the chief constituent of flint and quartz, namely, silicon, must now be admitted to share in this power, and likewise in the ability to form other remarkable compounds that it will be necessary for me to refer to in the course of this lecture.

In selecting this subject, I have done so in part because this most promising field of research, opened up by the labours of Wöhler, Buff, Friedel, Crafts, Ladenburg, and others, has hitherto been but little cultivated in this country, and therefore probably possesses some novelty for the audience I have the honour to address; and also because we find in this new branch of chemical investigation most interesting illustrations of the advantage we may derive from the cautious use of the argument from analogy.

As a preliminary to the inquiry I propose, we may consider very briefly the chemical nature of flint and quartz.

The word "flint" is of very ancient origin, and was often used to indicate any particularly hard rock. In this sense it is employed several times in the Old Testament—first, in the Book of Deuteronomy, viii. 15, and in Psalms, cxiv. 8, where the rock struck by Moses is said to be "of flint." We now use the term to distinguish a well-known uncrystalline mineral, which can be easily shown to be a chemical compound of two so-called elementary forms of matter—oxygen and silicon.

Flint is identical in chemical composition with quartz or rock-crystal, though physically different, as you will perceive by reference to the fine specimens on the table, kindly lent by Mr. Bryce

M. Wright, the well-known mineral collector. The common name of "silica" is given to the chemical compound, and the terms flint, agate, quartz, rock-crystal, are reserved for the forms in which we meet with this remarkable substance in nature.

Having cleared our ground so far, we have to find how the oxygen may be separated from any of those forms of silica, and the element silicon may be obtained. This cannot be directly accomplished, but by indirect means we can obtain the desired results. I have here a quantity of finely-divided flint mixed with some powdered fluor-spar; when I pour oil of vitriol on the mixture, and apply heat, a colourless gas is obtained, which, when passed into water, produces a highly acid and gelatinous liquid. The gas is a compound of the element fluorine, with silicon—the tetrafluoride of silicon—and this, when brought in contact with water, produces an acid called hydrofluosilicic and a quantity of gelatinous hydrate of silica.

The clear acid liquid, when treated with caustic soda, yields this white salt, the fluosilicate of sodium, from which we directly obtain the silicon, as you see, by simply heating with some metallic sodium. In this case, the sodium replaces the silicon, the latter separating, as you observe, in the tube as a dark brown substance.

Having thus prepared silicon from flint, we are in a position to compare it with carbon, to trace out the analogies which subsist between them, and then to show that some of the alcoholic and other *compounds* of carbon have their strange and interesting analogues in a silicon series.

First, then, we shall compare the elements themselves.

We meet with nearly pure carbon under the well-known forms of charcoal, graphite, and diamond.

We can easily prepare the corresponding varieties of silicon—the amorphous, the graphitoidal, and adamantine. With the aid of the phengascope, I shall now project on the screen images of specimens, in order that you may compare the varieties side by side. I have also on the table a very fine specimen of crystallized silicon, for which I have to thank Messrs. Hopkin and Williams. [The lecturer showed a greatly magnified image of a fine crystallized diamond on the screen; a number of other specimens were exhibited in the same way.]

As might be anticipated, the specific gravity of carbon is lowest in charcoal and highest in diamond. Corresponding differences in specific gravity are observed between the varieties of silicon. The two elements also correspond remarkably in variations of specific heat, with different states of aggregation. The specific heat of the diamond is lower than that of graphite, and the specific heat of adamantine silicon is lower than that of the graphitoidal variety.

Passing now from the points of physical resemblance between carbon and silicon, I shall dwell more particularly on the chemical relations of the two elements.

We are familiar with the fact that carbon burns in oxygen, producing, in an excess of that gas, the well-known gaseous oxide of carbon, commonly called carbonic acid; charcoal or coke burn readily in oxygen, while graphite is consumed with considerable difficulty, and the diamond is still more difficult of combustion. Amorphous silicon burns as easily in oxygen as charcoal, and forms the oxide silica, the same oxide that we find as flint or quartz. [Experiments exhibited.]

In this way we can reproduce, so far as composition is concerned, the substance from which we originally obtained the silicon for our experiments. Now, though amorphous silicon is easily burnt, the graphitic and adamantine varieties of the element resemble the corresponding forms of carbon in difficult combustibility. Crystalline silicon may be raised even to a white heat in oxygen gas without burning.

Unlike carbon, silicon in any of its forms easily combines directly with chlorine, producing the liquid chloride which I have in this tube. This is a very volatile body, boiling at 50° C., and is half as heavy again as water. It can also be prepared from silica by heating to full redness the finely-divided oxide and carbon in a current of chlorine. In composition this chloride is the silicon representative of tetrachloride of carbon.

In addition to this chloride of silicon, the discovery of which we owe to Berzelius, another has very recently been obtained by Friedel, which corresponds to a well-known carbon hexachloride.

We next pass to a compound of silicon with hydrogen. It may be prepared in a pure state by means of a rather complex reaction I shall have presently to refer to; but we can easily obtain the impure gas by Wöhler's method, in treating a compound of silicon and magnesium with hydrochloric acid. We thus obtain a colourless, spontaneously inflammable gas, which burns with a bright light on contact with the air. In its pure condition, siliciuretted hydrogen is not spontaneously combustible at ordinary pressure, but in a slightly rarefied atmosphere it easily inflames. The compositions of these silicon and carbon compounds are shown in this table:—

| | | |
|---|---|---|
| $SiO_2$ | Oxides. | $CO_2$ |
| $SiCl_4$ | Chlorides. | $CCl_4$ |
| $Si_2Cl_6$ | ,, | $C_2Cl_6$ |
| $SiH_4$ | Hydrides. | $CH_4$ |

The siliciuretted hydrogen is evidently the chemical analogue of marsh-gas, the tetrahydride of carbon.

It is usual to regard marsh-gas as the typical carbon compound from which some alcoholic series may be supposed to spring, and, in fact, all the alcohols belonging to the group of which the well-known wood-spirit and spirit of wine are the chief members, are commonly regarded as derivatives of marsh-gas, in which a part, or all, the hydrogen has been replaced by one or more compound radicals, such as hydroxyl, methyl, ethyl, propyl, &c. In these cases, carbon of the marsh-gas is the grouping element of the compound, or that constituent which serves to bind together the different materials of which the molecular edifice is constructed. In the same way the silicon in siliciuretted hydrogen may be shown to be the nucleus round which can be grouped hydroxyl, methyl, ethyl, &c., so as to form the alcohols whose compositions I shall presently have to refer to. Several of the less complex terms are still wanting, but their existence is rendered highly probable by the occurrence of bodies bearing the same close relation to the unknown alcohol that marsh-gas bears to wood-spirit, or the acid of vinegar (acetic acid) to common spirit of wine. As we ascend in the series, however, we meet with the true alcohols, in which silicon takes the place of carbon as the grouping element.

It must be here admitted, however, that no well-defined alcohols have yet been discovered in which silicon acts in any other way than as the nucleus of the compound; carbon radicals in all these cases playing the subordinate parts. But we have every reason to expect that complex alcohols containing silicon only will yet be obtained as researches extend.

In 1857 Buff and Wöhler obtained a volatile fuming liquid on heating crystalline silicon nearly to redness in a current of dry hydrochloric acid gas. The precise nature of this liquid was unknown until 1871, when Friedel and Crafts published the results of their admirable researches upon Buff and Wöhler's liquid, and showed that it was a mixture of chloride of silicon with a new body, which proved to be the strict chemical analogue of our well-known chloroform, silicon replacing carbon.

$SiHCl_3$ Chloroforms. $CHCl_3$

This body is a colourless, mobile, and very volatile liquid boiling at 35° C. I have a quantity of it in this tube. One of its most remarkable properties is that of exploding with great facility when its vapour is mixed with air. I shall now show you the experiment. Ordinary chloride of silicon does not afford an explosive mixture when its vapour is mingled with air. [Experiments shown.]

When this remarkable body is made to unite with anhydrous alcohol, a colourless ethereal liquid is obtained on distillation, having an agreeable odour, and a boiling-point at 134° C. This body is strictly analogous in composition to a substance obtained by Williamson and Kay, by acting on ordinary chloroform with sodium alcohol.

These ethers may each be regarded as derived from a glycerine or triatomic alcohol, as shown below. Neither of these alcohols has as yet been isolated.

$$Si\left\{\begin{matrix}H\\OC_2H_5\\OC_2H_5\\OC_2H_5\end{matrix}\right. \qquad \text{Formic ethers (tribasic).} \qquad C\left\{\begin{matrix}H\\OC_2H_5\\OC_2H_5\\OC_2H_5\end{matrix}\right.$$

$$Si\left\{\begin{matrix}H\\OH\\OH\\OH\end{matrix}\right. \qquad \text{Glycerins.} \qquad C\left\{\begin{matrix}H\\OH\\OH\\OH\end{matrix}\right.$$

By the action of sodium on the silicon ether just referred to we can obtain siliciuretted hydrogen in a state of purity. This is the only known mode of obtaining the pure compound.

Returning to the silicon chloroform, about whose chemical nature we can now have little if any doubt, we next have to inquire in what direction, and how far, we can pursue the analogy between the great pain-killer, discovered almost simultaneously by Soubeiran and the illustrious chemist of Giessen, who has so recently passed away from amongst us, and the curious body that we can obtain indirectly from flint or other form of silica in the manner I have described.

Ordinary chloroform is well known to be closely allied to common wood-spirit, or methyl alcohol, in a way that will be evident on comparing the formulæ. In fact, chloroform is easily obtained by treating wood-spirit with bleaching-powder. We cannot in any simple way reverse this process and prepare wood-spirit from chloroform, but we can do something in this direction, for we are able by the action of caustic potash to obtain from chloroform formic acid; a body which is one of the most remarkable products of oxidation of wood-spirit. The relation of formic acid to the alcohol is shown in the table below; and it is there further pointed out that this formic acid should yield an anhydride—a body capable of producing the acid by union with the elements of water. This anhydride is not known, but the importance of suggesting its existence will appear in a moment.

| | | |
|---|---|---|
| $\mathrm{Si}\begin{cases}\mathrm{H}\\\mathrm{H}\\\mathrm{H}\\\mathrm{OH}\end{cases}$ | Methyl alcohols. | $\mathrm{C}\begin{cases}\mathrm{H}\\\mathrm{H}\\\mathrm{H}\\\mathrm{OH}\end{cases}$ |
| $\mathrm{Si}\begin{cases}\mathrm{H}\\\mathrm{O}\\\mathrm{OH}\end{cases}$ | Formic acids. | $\mathrm{C}\begin{cases}\mathrm{H}\\\mathrm{O}\\\mathrm{OH}\end{cases}$ |
| $\mathrm{Si}\begin{cases}\mathrm{H}\\\mathrm{O}\\\mathrm{O(SiOH)}\end{cases}$ | Anhydrides. | $\mathrm{C}\begin{cases}\mathrm{H}\\\mathrm{O}\\\mathrm{O(COH)}\end{cases}$ |

Regarding silicon chloroform from the same point of view, analogy would lead us to look for a simple silicon alcohol similar to wood-spirit or methyl alcohol; but we would not expect the silicon chloroform easily to yield this alcohol directly, though we would be justified in hoping that an acid, corresponding to formic acid, might be obtained. As a matter of fact, no such alcohol has as yet been prepared even indirectly; but a corresponding acid is very readily produced, and more than this, for the anhydride at present wanting in the carbon series is found in that of silicon. If I pass the vapour of silicon chloroform into water nearly ice-cold, a white solid body is obtained without any evolution of hydrogen, and an acid liquid produced. The white solid then collected, washed, and dried at a low temperature, forms a white inflammable powder, which was first described by Buff and Wöhler. Friedel and Ladenburg have shown that this remarkable body is the anhydride of the silico-formic acid. According to the results of my own investigations, the acid liquid to which I referred just now contains, in addition to hydrochloric acid, the true silico-formic acid—a body possessing nearly as energetic reducing properties as the corresponding acid derived from wood-spirit. I shall now demonstrate these facts. [The lecturer then exhibited the experiments referred to.]

It will naturally be asked whether the silicon chloroform is capable of acting as an anæsthetic like ordinary chloroform. But it is only necessary to bear in mind the fact that it is very easily decomposed by water into gelatinous matter, and highly corrosive hydrochloric acid, in order to understand that its inhalation would be attended by the speedy destruction of the lungs of any person persisting in the experiment.

Starting from silicon chloroform, then, we have been led, by analogical reasoning in the first instance, to infer the existence of a simple silicon alcohol precisely corresponding to wood-spirit. On testing this induction by experiment, we have obtained answers which are, so far as they go, altogether favourable to the view just stated. In fact, the results are as satisfactory as they can be short of the discovery of the silico-methyl alcohol.

I shall now endeavour to strengthen this position by showing that the existence of three higher members of the alcoholic series has been rendered highly probable by the discovery of closely-related bodies, though the alcohols themselves have not been isolated; and, finally, I shall show that the alcohols of still higher terms have actually been obtained.

In the course of their elaborate and able investigation of silicon compounds, Friedel and Crafts discovered that chloride of silicon easily acts upon common alcohol, as I have already mentioned, producing a body which Friedel and Ladenburg have recently shown to be easily attacked by a mixture of sodium with a curious substance contained in this tube—zinc-ethyl. The product, when treated with caustic potash, yields a body which bears the same relation to silico-propyl alcohol that formic acid does to wood-spirit. This relationship is shown in the formulæ—

$$\mathrm{Si}\left\{\begin{matrix}C_2H_5\\ H\\ H\\ OH\end{matrix}\right. \qquad \text{Propyl alcohols.} \qquad \mathrm{C}\left\{\begin{matrix}C_2H_5\\ H\\ H\\ OH\end{matrix}\right.$$

$$\mathrm{Si}\left\{\begin{matrix}C_2H_5\\ O\\ OH\end{matrix}\right. \qquad \text{Propionic acids.} \qquad \mathrm{C}\left\{\begin{matrix}C_2H_5\\ O\\ OH\end{matrix}\right.$$

This silico-propionic acid is in this tube, and is a white combustible powder, like the silico-formic anhydride I have already shown to you. It is soluble in warm caustic potash, but not in caustic soda; by which character it can be distinguished from silica. It is only necessary to state that it can be obtained in aqueous solution, and in the pure state, by Professor Graham's valuable dialytic process.

In the amyl term, neither alcohol nor acid are yet known; but by the action of zinc-methyl on chloride of silicon we can obtain a light, colourless, and very volatile liquid, which is silicon-methide, a body that may, for reasons which will presently appear, be fairly considered to stand in the same relation to silico-amyl alcohol that marsh-gas does to wood-spirit. Thus—

$$\mathrm{Si}\left\{\begin{array}{l}C_4H_9\\H\\H\\H\end{array}\right. \qquad \text{Amyl hydrides.} \qquad \mathrm{C}\left\{\begin{array}{l}C_4H_9\\H\\H\\H\end{array}\right.$$

$$\mathrm{Si}\left\{\begin{array}{l}C_4H_9\\H\\H\\OH\end{array}\right. \qquad \text{Alcohols.} \qquad \mathrm{C}\left\{\begin{array}{l}C_4H_9\\H\\H\\OH\end{array}\right.$$

The researches of Friedel and Crafts have made us acquainted with a body which may also probably be regarded as the hydride corresponding to silico-hexyl alcohol. This compound is prepared by the action of zinc-methide and zinc-ethide in chloride of silicon.

$$\mathrm{Si}\left\{\begin{array}{l}C_5H_{11}\\H\\H\\H\end{array}\right. \qquad \text{Hexyl hydrides.} \qquad \mathrm{C}\left\{\begin{array}{l}C_5H_{11}\\H\\H\\H\end{array}\right.$$

$$\mathrm{Si}\left\{\begin{array}{l}C_5H_{11}\\H\\H\\OH\end{array}\right. \qquad \text{Hexyl alcohols.} \qquad \mathrm{C}\left\{\begin{array}{l}C_5H_{11}\\H\\H\\OH\end{array}\right.$$

Having, therefore, stated the grounds for inferring the existence of silico-propyl, silico-amyl, and silico-hexyl alcohols, I shall now pass at once to the second class of evidence, and show that the alcohols of still higher terms can actually be prepared.

In referring to the preparation of silico-propionic acid, it was stated that when chloride of silicon acts upon absolute alcohol a body is obtained which, on treatment with zinc-ethyl and sodium, yields an ethereal product from which silico-propionic acid can be obtained by treatment with caustic potash. If, however, instead of using the caustic alkali we continue the action of zinc-ethyl and sodium, decompose the products with water in sealed tubes, and distil, a liquid is obtained which contains one of the "alcohols from flint" we are in search of. In this tube I have a small quantity of the alcohol, and here you will find its composition stated—

$$\mathrm{Si}\left\{\begin{array}{l}C_2H_5\\C_2H_5\\C_2H_5\\OH\end{array}\right. \qquad \text{Heptyl alcohols.} \qquad \mathrm{C}\left\{\begin{array}{l}C_2H_5\\C_2H_5\\C_2H_5\\OH\end{array}\right.$$

You will observe that it is the silico-heptyl alcohol precisely corresponding to a simple carbon alcohol recently discovered by Nahapetian, both being *tertiary* alcohols. We owe to Ladenburg the discovery of this lowest known term of alcohols containing silicon.

As you can observe it is a colourless liquid, not unlike the ordinary alcohol of wine. It is insoluble in water, but easily dissolved by spirit and ether. Chemically it acts just like any of the other alcohols, producing ethers, and dissolving the alkali metals to form sodium or potassium alcoholates. When common spirit burns you are aware that its flame is nearly colourless, but I shall now burn some of our alcohol from flint, and you will find, particularly when we feed the flame with oxygen, that a bright light is emitted.

Clearly defined though this alcohol is, it does not stand alone, for at least one other compound of the same order is known. It was suggested in 1870, by Friedel and Crafts, that silicon ethide—a body easily prepared by the action of chloride of silicon on zinc ethide—might be regarded as the hydride of silico-nonyl, and should stand in the same relation to an alcohol that marsh-gas does to common wood-spirit, or ethyl hydride to ordinary alcohol. This happy idea, when put to the test of experiment, was fully justified by the result, for, on treating silicon ethide in essentially the same manner that we should adopt in preparing wood-spirit from marsh-gas, a colourless liquid, lighter than, and insoluble in, water is obtained. The boiling point of this body is 190° C. It yields an ether with acetic acid, dissolves sodium, forming an alcoholate, and, in fact, conforms to the general habits of the alcohols of the series to which common spirit belongs. The compositions of these bodies are thus represented—

$$\mathrm{Si}\left\{\begin{array}{l}C_8H_{17}\\ H\\ H\\ H\end{array}\right. \qquad \text{Nonyl hydrides.} \qquad \mathrm{C}\left\{\begin{array}{l}C_8H_{17}\\ H\\ H\\ H\end{array}\right.$$

$$\mathrm{Si}\left\{\begin{array}{l}C_8H_{17}\\ H\\ H\\ OH\end{array}\right. \qquad \text{Alcohols.} \qquad \mathrm{C}\left\{\begin{array}{l}C_8H_{17}\\ H\\ H\\ OH\end{array}\right.$$

It is thus shown to be precisely similar to the nonyl alcohol prepared by Pelouze and Cahours from American petroleum.

Ladenburg has very recently advanced even beyond the point we have now reached, and has shown that the chloride of silicon can be made to yield two ethers, which correspond, as I may suggest, to silico-nonyl diatomic and triatomic alcohols. Thus—

$$\mathrm{Si}\left\{\begin{array}{l}C_2H_5\\ C_2H_5\\ OC_2H_5\\ OC_2H_5\end{array}\right. \quad \text{or} \quad \mathrm{Si}\left\{\begin{array}{l}C_8H_{17}\\ H\\ OH\\ OH\end{array}\right.$$

$$\mathrm{Si}\left\{\begin{array}{l}C_2H_5\\ OC_2H_5\\ OC_2H_5\\ OC_2H_5\end{array}\right. \quad \text{or} \quad \mathrm{Si}\left\{\begin{array}{l}C_8H_{17}\\ OH\\ OH\\ OH\end{array}\right.$$

In all the preceding compounds it will have been noted that but one atom of silicon is present, and though, as I pointed out in the earlier part of this lecture, the silicon in these cases occupies the chief position as the grouping element, we should much like to see silicon uniting with silicon, and forming a more condensed compound with hydrogen. Happily, however, very important evidence, even upon this point, is forthcoming, for Friedel and Ladenburg have discovered corresponding hexa-chloride, iodide, and bromide of silicon, and treatment of the hexa-iodide with zinc-ethyl enables us to obtain the ethide whose formula is given in this table—

$$\begin{array}{ccc} \text{Hydride.} & \text{Chloride.} & \text{Ethide.} \\ \begin{matrix}\mathrm{Si}\\ \mathrm{Si}\end{matrix}\left\{\begin{matrix}\mathrm{H}\\ \mathrm{H}\\ \mathrm{H}\\ \mathrm{H}\\ \mathrm{H}\\ \mathrm{H}\end{matrix}\right. & \begin{matrix}\mathrm{Si}\\ \mathrm{Si}\end{matrix}\left\{\begin{matrix}\mathrm{Cl}\\ \mathrm{Cl}\\ \mathrm{Cl}\\ \mathrm{Cl}\\ \mathrm{Cl}\\ \mathrm{Cl}\end{matrix}\right. & \begin{matrix}\mathrm{Si}\\ \mathrm{Si}\end{matrix}\left\{\begin{matrix}\mathrm{C_2H_5}\\ \mathrm{C_2H_5}\\ \mathrm{C_2H_5}\\ \mathrm{C_2H_5}\\ \mathrm{C_2H_5}\\ \mathrm{C_2H_5}\end{matrix}\right. \end{array}$$

It is not improbable that in the last-named compound we have the starting-point of a new series of still more complex bodies, analogous to derivatives of olefiant-gas rather than to those of marsh-gas.

I trust you will now admit that the case I proposed to lay before you has been made out—namely, that we can obtain some alcohols indirectly from flint or other form of silica, and that we have solid ground for inferring the existence of many others.

A rich and beautiful field for chemical research appears to lie before us in tracing out the analogies between the compounds of carbon and silicon, and recognising the chemical representatives of many of the most complex "organic compounds" in the native silicates which form so large a part of the crust of this earth.

Hitherto in this lecture I have avoided reference to subjects not directly connected with the matter in hand, but, before concluding, I would refer, necessarily in a very few words, to some inquiries in this department of chemistry which have been carried on in the laboratory of the Royal Dublin Society. First, however, allow me to perform an experiment. I have here a glass jar filled with dry ammoniacal gas; when I drop it into some chloride of silicon, a white compound is obtained. This body was discovered by Persoz in 1830, and the composition he assigns to it would, as ably suggested by Dr. Hofmann, represent a mixture of sal-ammoniac with the hydrochlorate of silicon-guanidine. If this white body be ignited strongly in a closed vessel, an infusible white substance remains, which has been examined by Deville and Wöhler, who have shown that it contains silicon and nitrogen, and that the same or similar body can be produced by intensely heating crystalline silicon in an atmosphere of nitrogen.

My examination of this curious body has led me to the conclusion that it is the silicon analogue of cyanogen—a well-known compound of nitrogen with carbon, and the chief constituent of the deadly poison prussic acid. This body, though little affected even at a very high temperature, in the absence of moisture is easily decomposed by steam—silica, ammonia, and hydrogen resulting. A similar decomposition is effected by heating with soda-lime. I may add that, when this singular compound is fused with a small quantity of carbonate of potassium, cyanide and silicate of the metal are produced; in this case, carbon appears to displace silicon.

If, in the experiments with ammoniacal gas, we substitute silicon chloroform for the silicic chloride, a body is obtained from which this compound of silicon and nitrogen can be easily extracted. Now we know well that when ordinary chloroform is heated with ammonia gas, chloride and cyanide of ammonium are obtained, and that under certain circumstances a body called *para-cyanogen* is also produced. Analogy would lead us to anticipate that silicon chloroform would react in a similar manner, and the facts I have hitherto observed justify this inference.

It would be out of place to pursue this subject here, as the results of the inquiry referred to have not yet been published. I have, therefore, now simply shown to you one of the chief bodies to which interest belongs, and ventured to point out the relationship I believe it to bear to some of the well-known and remarkable compounds of carbon with nitrogen.

In concluding this lecture, I need simply remind the audience I have the honour to address that the practical value of scientific research is rarely apparent at first. Who could have suspected that the benzole discovered by the venerable philosopher whose name is so inseparably connected with this Institution, would have proved, in the able hands of Perkin and of Hofmann, the chief source of many of the exquisite dyes now largely manufactured in this country? Yet in this, as in a hundred of other instances, the small and apparently useless scientific seedling has gradually expanded into the strong tree, yielding its rich store of useful fruit. Let us hope that a similar future awaits some of the alcohols from flint which have been referred to this evening, and that, in pursuing our studies of the silicon analogues of the more complex carbon compounds, we may be led to appreciate more fully than we have hitherto done the admirable economy and harmony of Nature. [J. E. R.]

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## GENERAL MONTHLY MEETING,

Monday, May 5, 1873.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:—

George Busk, Esq. the Treasurer,
Warren De la Rue, Esq. D.C.L. F.R.S.
The Lord Lindsay,
William Spottiswoode, Esq. LL.D. F.R.S. the Secretary.

Mrs. Bergheim,
Maures Horner, Esq.
Edwin Lawrence, Esq. LL.B. F.R.A.S.
Hugh Matheson, Esq.
Miss Frederica Scott,
Alexander Young Stewart, Esq.

were *elected* Members of the Royal Institution.

The following communication to the family of the late Dr. Bence Jones was read and adopted :—

"The Managers of the Royal Institution were instructed by a unanimous vote at the recent Annual Meeting of its Members, to convey to the family of their late Honorary Secretary, Dr. Henry Bence Jones, the expression of their strongest sympathy in their sorrow for the great loss they have sustained by his death,* and to transmit to them also the expression of the deep sense the Members entertain of the value of the great and eminent services which the Institution has received from their late Honorary Secretary during his long official connection with it."

John Tyndall, Esq. LL.D. F.R.S. was re-elected Professor of Natural Philosophy.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

*Antiquaries, Society of*—Proceedings, Vol. V. No. 6. 8vo. 1872–3.
*Astronomical Society, Royal*—Monthly Notices, Vol. XXXIII. No. 5. 8vo. 1873.
*British Architects, Royal Institute of*—Sessional Papers, 1872–3, Nos. 9, 10. 4to.
*Chemical Society*—Journal for April, 1873. 8vo.
*Cox, Edward W. Esq. Sergeant-at-Law, M.R.I. (the Author)*—What am I? A Popular Introduction to Mental Philosophy and Psychology. Vol. I. 8vo. 1873.
*Editors*—American Journal of Science for April, 1873. 8vo.
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## WEEKLY EVENING MEETING,

Friday, May 9, 1873.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

MOUNTSTUART E. GRANT DUFF, Esq. M.P.

### *A Fortnight in Asia Minor.*

SLOWLY the lights of Stamboul died out in the distance, and we glided away into the silent Sea of Marmora. Far off to the left, and not to be visited by us upon this occasion, lay, at the bottom of its gulf, Nicomedia, where Diocletian resigned the throne of the world, and retired to grow cabbages at Salona. There, likewise, was Broussa, at the foot of the Mysian Olympus, which had so tantalizingly kept its head shrouded in the clouds all the time we were in Pera, although our windows opened straight upon it. We had made various plans for going to Broussa, which attracted us not only by its beauty, but as the first centre of the Ottoman power in these regions, but they all broke down before the exigencies of time, that stern controller of autumn holidays.

With Broussa went all chance of seeing Nicæa, the seat of the first General Council, and one of the most famous places in the history of the first Crusade; partly from its stout resistance to the Christians, and partly from the fact that Walter the Penniless died pierced by seven arrows before its walls.

We passed on, however, as I say, although well-nigh every mile of these historic shores invited us to stay. There at Libyssa, Hannibal had ended by poison his earth-shaking career; and there near Moudania, the port of Broussa, a more modern warrior, quite as great in his way—Count von Moltke—had run a considerable chance of being drowned, in the year 1836, as he himself has told us in his pleasant letters on Turkey.

These places, however, and many others, though very near us, we could behold only with the eye of the mind, for brilliant as was the starlight aided by a young moon, it enabled us to see but a very short distance on either side of our vessel's track.

In the first grey of the morning I came on deck, and found that we had already left behind the marble island of Marmora, which gives its modern name to the ancient Propontis, the peninsula of Proconnesus, at the landward end of which lay Cyzicus, the mouth of the Granicus, made memorable by Alexander the Great's battle; Gallipoli, so famous in the history of the Crimean War; and Lampsacus, which was one of the cities assigned by the Great King for the support of Themistocles.

Passed also, no less, was a spot which some might have cared even more to see, the hillock to which tradition has attached the name of the Tomb of Memnon, that mysterious personage who, as the legend said, came from some far-away country to assist the Trojans, and whose last resting-place was kept watered from the river Sangarius by the birds one still sees flying about on these seas, which the ancients called "Memnoniæ aves," and the moderns less gracefully term the *âmes damnés.*

The sun had risen over the hills on the Asiatic shore before we ran between Sestos and Abydos, and drew near to the town which Europeans call the Dardanelles, but which the Turks, with their genius for the commonplace in names, call Tchanak Kalesi, or Pottery Castle, from the flourishing manufacture of earthenware which exists there. Presently a boat came alongside, bringing one of the English residents in the town, to whose kind keeping we had been consigned by friends. Here, after a visit to the Governor, a friendly and hale old man who commanded the Turkish fleet when the Allies engaged the seaward batteries of Sebastopol, we spent half-an-hour in inspecting the far-famed Castle of Asia, whose monster guns still bear the marks of the balls which struck them when Duckworth, not too wisely, ran up past them to Constantinople. * * * * The military Pasha gave us a large boat with fourteen rowers, and we were soon afloat. After a row of about two hours and a half, we approached a quiet little bay with a shelving shore, white cliffs to the left, and a sandy hill to the right. We touched land and stood upon the soil of the Troad, for the sandy hill to the right was no other than the Eastern face of the Rhætean promontory.

It was curious to think, as we drew near the beach, how many and how different were the travellers in whose wake we were following. Hither turned aside Xerxes on his way to attack Athens, and Alexander on his way to conquer Persia; so did Mindarus the Spartan, the hero of the famous and characteristically laconic despatch; Ovid came also as a youth with his tutor Macer; and Germanicus; and Julia, the daughter of Augustus, who, by-the-by, was all but drowned in the Scamander. These are a few, and only a few, of the famous personages of antiquity whom we know to have visited the Troad for the same purpose for which we were now landing on its shores.

And no wonder, for this little nook of a continent has been of vast importance as well historically as poetically. I say historically, because whatever may have been the foundation in fact for the tale of Troy divine as told by Homer, it is at least clear that the father of history considered the Trojan War to have been one of the most important and determining events in that long series of actions and reactions between East and West on which he dwells so much, and which continuing through the ages has enshrined a Semitic religion on the Tiber and the Thames, and has made a Hampshire country gentleman an incomparably greater Asiatic ruler than ever was Darius or Artaxerxes—Tamerlane or Nadir Shah.

But great as is the historical importance of the Troad, its poetical importance is far greater. Eliminate the story of the War of Troy with all that came of it, and it is startling to see how much of Greek and even of Roman literature will disappear. The Iliad and Odyssey as they vanish will take with them the Agamemnon, the Chœphoræ, the Eumenides of Æschylus, the Electra, the Philoctetes, and the Ajax of Sophocles, the Æneid of Virgil, some of the best odes of Horace, some of the finest poems of Ovid, with I know not how much else.

How much of even our contemporary poetry has sprung from this region. Wordsworth has written little finer than Laodamia; Tennyson nothing finer than Ulysses; Morris nothing finer than the Death of Paris.

Mounting the horses which had been sent to meet us at the landing place, we rode up a short ascent, the edges of which were made bright by the yellow flowers of the *Inula viscosa*, a showy plant which is very conspicuous in Attica in the autumn, but which I did not observe at any other point of the Troad—where, indeed, almost all herbaceous vegetation was utterly dead and burnt up.

Soon we were standing by the wild fig-tree which has struck its roots deep into the masonwork of the central chamber in the tumulus of Ajax. That central chamber has long since been opened and affords no new light to the antiquary. From it we looked across the mouth of the Scamander to the Sigæan promontory, and all over the line of flat shore where Homer has placed the station of the Grecian ships. I was surprised to find the Scamander so considerable a stream even at this, the driest moment of an exceptionally dry season (do not be surprised by my adjective), for although exceptionally wet here, 1872 was exceptionally dry in the Troad. I have seen both the Simois and the Scamander described as "historic rivulets," but that is a mistake, at least with regard to the second river.

From the tomb of Ajax we descended, and the sun being already not far from its setting, set off, as hard as our horses would carry us, across the plain and over the dry bed of the Simois to Hissarlik, one of the two places which archæologists please themselves by supposing the site of Troy. It is at this point that Dr. Schliemann, an enthusiastic and original German inquirer, is making immense excavations, and is, as he confidently believes, unveiling stone by stone the old poetic city. Here we tarried as long as the light served us, and then rode through a wild region covered with low brushwood, and dotted here and there by the fires of shepherds, to a farm built on the site of the ancient Thymbra, and belonging to one of the gentlemen who had accompanied us from the Dardanelles. * * * *

The next morning we made an early start, and followed the course of the Thymbrius to its junction with the Scamander, passing on our right the great tumulus known as the Hanai Tepe, that is, tumulus of the place where there have been ruins, which is supposed by some, as, for instance, by the late Mr. Senior, to have been the burial-place of the undistinguished mass of warriors who fell before Troy; an opinion to which I should, as at present advised, not be inclined to subscribe. Certain, however, it is that it must record some considerable engagement in these parts. * * * *

The name of this place makes one think of the well-known phrase of Lucan, "Etiam periere ruinæ,"—which, as it happens, occurs in a passage describing this very district, in connection with an imaginary, but well-imagined, visit of Julius Cæsar's. * * * *

One of the ladies of our party handed to me a translation of these lines, which, whatever its merits may be, represents the characteristic terseness of the original much better than Rowe's.*

"Lover of fame he seeks Sigæum's sand,
Rhæteum honoured with a hero's grave,
Where Simois' waters wander thro' the land.
(To many a shade there genius glory gave;)
He roams around the burnt-out Trojan halls
And views the relics of Apollo's walls.

"Home of Assaracus! the barren tree
The rotten trunk above thy hearth have grown,
The temples of thy gods are like to thee—
The worn-out roots scarce cling around thy stone;
Thy ruins ruined, and the thickets shroud
In briar and brushwood, Pergamus the Proud.

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* "Sigeasque petit, famæ mirator, arenas,
Et Simoentis aquas, et Graio nobile busto
Rhæteon, et multum debentes vatibus umbras.
Circuit exustæ nomen memorabile Troiæ,
Magnaque Phœbei quærit vestigia muri.
Iam silvæ steriles, et putres robore trunci
Assaraci pressere domos, et templa deorum
Iam lassa radice tenent; ac tota teguntur
Pergama Dumetis: etiam periere ruinæ.
Adspicit Hesiones scopulos, silvasque, latentes
Anchisæ thalamos: quo judex sederit antro;
Unde puer raptus cœlo; quo vertice Nais
Luserit Œnone! nullum est sine nomine saxum.
Inscius in sicco serpentem pulvere rivum
Transierat qui Xanthus erat. Securus in alto
Gramine ponebat gressus: Phryx incola manes
Hectoreos calcare vetat. Discussa jacebant
Saxa, nec ullius faciem servantia sacri
'Herceas,' monstrator ait, 'non respicis aras?'" * * * *

> "He sees the woods that hid Anchises' suit,
> The rock to which Hesione was tied,
> The cave where Paris gave the golden fruit,
> Where Ganymede was snatched. Olympus' pride,
> The summit where the nymph Œnone played:
> No stone without its name, no wood, no glade.
>
> "Unconscious now the dusty bed he passes,
> Where creeps divine Scamander's scanty wave,
> The Phrygian cries, 'Tread not those long dank grasses;
> Spurn not what was the mighty Hector's grave.
> See,' says the guide, 'behold the Hercean fane,
> The scattered stones nor form nor place retain.'"

The Thymbrius is at the best of times little more than a deep brook, and when we saw it, it was only a thread of water. Such as it is, however, it gives life to a great deal of vegetation, and is shrouded in a thicket of clematis, wild vine, and agnus castus, a beautiful shrub which is even in October made gay with many blossoms, and which a few weeks earlier must be a great ornament to every thicket in this country where there is a little water.

Soon we forded the Scamander, and reached by a long ascent the hill of Bounarbashi, the most usually received site of Troy. It is a long eminence overgrown with hardy brushwood and little else, for the rock comes to the surface in almost every part of it. The view on all sides is magnificent, extending over the whole Troad, over the islands of Tenedos, Imbros, and Samothrace, as well as over a considerable slice of the European shore of the Dardanelles. Turning to the south we had before us a very picturesque portion of the range of Ida, but not the portion to which I shall presently carry you. Below us the Scamander came out of the hills, flowing far down between well-wooded precipitous banks, and reminding me of familiar scenes in the north of Scotland. Bounarbashi is, as I have said, the usually received site of Troy. Now, I am not going to treat you to a dissertation upon that question, I am only going to express an opinion that, wherever Troy was, it most certainly was not at Bounarbashi. * * * * I defy any unprejudiced man to go up to Bounarbashi, and say that there ever was a city there at all on the scale of the Troy of Homer. At the far southern end of the hill, just above the valley, there was unquestionably a small Greek city, probably Gergis. There the excavations of the Austrian antiquary, M. Von Hahn, have laid bare the ancient walls; but M. Von Hahn found nothing on that site to point to any very remote antiquity, nothing that did not belong to a Greek city.

On the very top of the hill of Bounarbashi, and at the northern end, is a large tumulus, which has been christened, quite arbitrarily, the tumulus of Hector. Into this tumulus a deep trench was driven while we were on the spot, under the superintendence of Sir John Lubbock and of Mr. Frank Calvert, who is the great authority upon all questions connected with the Troad. A considerable body of men worked for three days, and got quite down to the living rock without finding anything of the slightest importance; but directions have been left to complete the investigation under careful superintendence, and to communicate the results.

Passing the little Turkish village of Bounarbashi, we rode down to look at the springs known as the Kirk Gheuz, or Forty Eyes, absurdly identified with the Scamander by Lechevalier, whose errors and misrepresentations were well exposed by the "Travelled Thane" in the sixth volume of the 'Edinburgh Review,' and by Mr. Charles Maclaren in his admirable book published in 1822, but of which I have only seen the enlarged edition published in 1863.

The water from these springs now finds its way to the sea at Besika Bay through a channel constructed in comparatively modern times. Mr. Maclaren believes, and it is a harmless superstition enough, that the marshes of which Homer speaks, as pasturing the flocks of Erichthonius, were those which now surround these springs, and which were, doubtless, much more extensive before an artificial watercourse was cut.

Next day we turned our horses' heads towards the sea, and retraced the route that we had followed in the dark on the day of our arrival in the Troad. I rode with our host, conversing with him upon his experiences as a farmer as we cantered along over the swelling eminences which border the plain of Troy, properly so called, covered with the prickly oak, and with another species of oak which produces the galls of commerce. Then, crossing the plain itself, we arrived at the site of Novum Ilium and the scene of Dr. Schliemann's excavations. Here we examined the ruins pretty carefully, and came to the conclusion that, whether Dr. Schliemann is or is not right in supposing he has found the site of Troy, he has certainly found the site of a city of vast antiquity; and here I will suspend my narrative for a few minutes in order to read to you, by the kind permission of Sir John Lubbock, a letter which he has just received from Dr. Schliemann, and which will give you some idea of that gentleman's work, and of the spirit which animates him.

TROY, *April* 10*th*, 1873.

Your Excellency is respectfully informed that, in company of Mrs. Schliemann, I have resumed the excavations on the 1st February, and have worked ever since with 120 to 150 labourers. Our first occupation was to dig up the site of the temple on the north side of the Pergamos, for I supposed that a substruction wall, which I had discovered there last year, would lead us to the ancient Trojan temple of Minerva. But the only result of this enormous excavation was to lay bare a large part of that wonderful substruction wall which once covered the whole of the N.E. slope of the hillock, whilst it is now 80 metres distant from the eastern, and 40 metres from the northern descent. There is certainly no other mount in the world which has increased in width to that extent, and the wonderful thing is here—that only a layer 2 metres in thickness belongs to the time of from about 700 before Christ until now, and all the remainder of the rubbish derives from the nations which had settled here before the arrival of the Greek colony.

Our second great excavation was that of a vast building, 24 metres broad by 100 long, which I had struck by accident in digging for the great tower in a S.E. direction. The numerous marble columns, &c., prove that this building had been in the Corinthian style, and the inscriptions I found there prove that it was the "Minerva" temple. Hoping to find in its depths the most ancient "Minerva" temple, we went on excavating it, and removed all its stones and pavements except the S. and N. walls and a reservoir. The foundations of the temple reach nowhere deeper than 2 metres, and from this depth off, pottery, weapons, in fact everything, are entirely different from anything you see in any museum. Please observe that the Minerva temple's *foundations* are just 6 metres above the great tower, on which I found a large house with eight chambers, and in one of the larger rooms the skeletons of two warriors with helmets on, both helmets broken, but their upper part is well preserved, and consists of a lower and an upper piece into which the *λόφος ἵππουρις* was fastened; both pieces are what Homer calls the *φάλος*, and therefore of the highest importance to science. With one of the skeletons I found a long lance; one of the skulls is broken, the other is well preserved, of large size, but very narrow. The house is built of small stones joined with earth, and on some parts of the inner walls you see remnants of the clay covering, which is usually painted yellow, but in one room it is white. In some chambers the floor is of stones, in others it has been of planks, as is shown by the fire-mark, which goes in a horizontal line all around the lower parts of the rooms; there is also one wall which forms a half-circle. I found in the house a vast quantity of beautiful pottery, also some idols of the owl-headed Ilian Minerva, and several goblets on which her owl's head has a helmet; also a number of vases on which the owl-headed goddess is represented holding her two arms upright. * * * *

If I had *no* other proof this owl-headed Minerva, which is constantly called *γλαυκῶπις* by Homer, would alone prove that I am digging in the ruins of the city to which Homer gives the name of Troy. It has been objected that if *γλαυκῶπις* means "with an owl's face," *βοῶπις* must necessarily mean "with an oxen face," and Juno must therefore have been represented with face of an ox. Certainly such has been the case; but it appears that already, in a pre-Homeric time, people began to find it ugly that the greatest of all goddesses should have the face of an ox, and they therefore gave her a human face with large eyes, which she must already have had in the time of Homer, because, as Juno had retained of her oxen face only the immense eyes, her former epithet *βοῶπις* continued to be in use, but thenceforth it merely meant "with large eyes," so that *this* epithet is also given by Homer to mortal women. But if your Excellency would dig up the ruins of the ancient *Ἡραῖον*, or temple of Juno, on Samos, to the virgin soil, he would beyond any doubt find these idols with the face of an ox, just as I find the *γλαυκῶπις* in the depths of Troy.

I also found in the house many form-stones for casting weapons and implements, several of them had forms for twelve lances and instruments. Further, a crystal *σκῆπτρον* button in the shape of a lion of beautiful workmanship. But the most curious thing I ever found here is a hippopotamus of terra-cotta, discovered in a depth of 7 metres, painted red, and of such a splendid workmanship that no artist now-a-days could make it better. At the foot of the ancient house is an altar made of a huge stone of mica slate, the upper part of which is cut in the shape of a crescent, and which is placed on a large table of the same stone, and below it is a gutter for the blood of the animals. On the north side, at the foot of the house, I find quite a labyrinth of ancient house-walls, all of which denote a tremendous conflagration. On the west side I have cleared the great tower as far as my frame house, at the foot of which, in a depth of 9 metres or 30 feet, I find, in the very tower, a gateway, which is paved with splendid tablets of hard white stones. The road in the gate *slopes* in a westerly direction towards the plain, and without any doubt, by following up this road, one comes to the Skæan gate, which cannot be further distant than 200 metres from this gate of the Pergamos, for its position seems to be clearly indicated by the formation of the ground. No sooner had I discovered yesterday that splendid gate, when I started a new gigantic excavation in order to bring to light the princely buildings to which the gate appears to lead, and in eighteen working days I shall have dug away the whole of that huge mass of rubbish which, as your Excellency remembers, separates the great wall from the tower and my magazine. * * * *

I hope my excavations here will be terminated by the 15th June, for I intend excavating Mycenæ and Olympia, and perhaps Delphi. I should be very happy to see your Excellency here *before* the end of May; certainly science would gain greatly by your Excellency's visit. I would gladly show the drawings of all the interesting objects found this year, for it is beyond human intellect to judge of mountains of rubbish without having seen what has come out of them, and particularly without having examined the pottery of each of the strata.—I remain, respectfully,

(Signed) DR. HENRY SCHLIEMANN.

*To His Excellency Sir John Lubbock, Bart., London.*

Descending from Hissarlik, we wandered for some time in the plain looking for a ford, which we never found, over a long, narrow channel having all the appearance of a small deep river, but really an arm of the sea, into which the Simois discharges its waters when it has any waters to discharge. At length we crossed it by a bridge, and fording the Scamander near its mouth, rode on to the tumulus of Achilles upon the Sigæan promontory, enjoying the spectacle of a perfect fleet of merchantmen running up the Hellespont with a fine breeze. Whether the name of Achilles is rightly or wrongly connected with this spot, we cannot doubt that it was here that Alexander came to visit the last home of his great predecessor; and it is the place that is described by Byron in some of the finest lines he ever wrote.*

A long and hot ride took us from this point back to Bounarbashi, whither we went to see how the excavations of the tumulus of Hector were getting on, and from which we enjoyed a distant view, not only of Lemnos, which we had seen the night before, but even of Athos in Macedonia, the holy mountain of the Greek church at the opposite side of the archipelago.

Here we lingered till the sun had set; then, picking our way slowly down the rough hill of Bounarbashi, we rode home by moonlight, crossing the Scamander, at a point where a close, heavy smell bore witness to the fact that, at certain hours and certain times, many parts of this district are dangerous from fever. Arrived at the farm, we found the dogs of the establishment very much on the alert, and soon discovered the cause, for not only were the jackals very near and very noisy, but a hyena had come prowling about to add his unlovely voice to their chorus. Thus ended our third day upon the plains of Troy.

From these scenes of poetic and legendary interest we passed to others whose importance is purely historical; for our next visit was to Alexandria Troas.

Retracing our steps to the bottom of the hill of Bounarbashi, and leaving the tumulus of Hector far on the left, we arrived after five hours' riding, chiefly through oak-scrub, at the great Vallonea forest, which spreads far and wide over and around the ruins of that place, which are still known as Eski Stamboul—Old Constantinople.

Alexandria Troas lies near the sea, and within view of all ships making for the mouth of the Dardanelles. Hence it was one of the first classical sites in these countries which became familiar to the people of the west. One large mass of building, which was peculiarly prominent, received from mariners the name of the Palace of Priam, though I need not say that Priam had nothing whatever to do with it. That building, however, although it still stands, has been shorn of its glories, for the Turks have used it as a quarry, and carried off many of the huge blocks, of which it was built, to Constantinople, and elsewhere. Alexandria Troas was founded by one of Alexander's generals, and received from another of them, who afterwards became possessed of it, the name of his great master; but in common parlance the word Alexandria appears to have been dropped, and we find it spoken of in the New Testament simply as Troas. It is a spot of peculiar interest to the European traveller, because it was here that St. Paul had that vision of a man of Macedonia begging him to come over to the opposite coast, which has had so important an influence upon the lives of us and of our ancestors for many hundred years. It was here, too, that he met St. Luke, who was destined to become his biographer, and who, through that biography, which we know as the Acts of the Apostles, has himself exercised so remarkable an influence upon Christian history. St. Luke was not, however, a native of Troas. He appears to have been a European, born at or settled in the seaport of Neapolis in Macedonia, still a place of some commercial importance under the name of Cavalla.

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* 'The Bride of Abydos,' Canto II., ii., iii., and iv.

We passed the night of the day on which we had visited the site of Alexandria Troas in a small town not far off, the "descendant bien descendu" of a city which was once, in the days of Constantine, within an ace of becoming the capital of the world. Here we were lodged in the house of an Armenian gentleman engaged in the Vallonea trade. The Vallonea of commerce, as some, perhaps, of you are aware, is largely used for tanning, and plays no inconsiderable part in the export trade of Asia Minor. It is the rough cup of the acorn of the *Quercus ægilops*, and its name is simply a corruption of the old Greek word for acorn, *βάλανος*. * * * *

Not far from Alexandria Troas there rises a hill, which forms a conspicuous object from most parts of the Troad, and is known to the natives as Chigri. This hill we were anxious to ascend, partly for the sake of the view, and partly to explore the ruins of the very considerable Greek city of Neandria, which stood upon the top of it. We were not disappointed in either purpose. The view is a very noble one, although Chigri is too distant from the plain of Troy, properly so called, to enable one to discern its features with sufficient accuracy, and the vast walls of Neandria are really a most surprising monument. How so large a place stuck on the top of an eminence, which might fairly think itself entitled to be called a mountain rather than a hill, was ever supplied with the ordinary articles of consumption, it is not easy to understand. As we descended we came upon a village of Yurucks. These people, who are true Turks in blood, lead a half or wholly nomad existence all through the regions of which I am speaking. This particular colony of Yurucks lived in hovels, but both before and afterwards we came upon some who lived in black tents, not in anything that could even aspire to the dignity of a hovel. The tent dwellers did not, however, seem to be by any means very poor. One of the ladies of our party penetrated into one of the tents where a woman was lying ill, and reported that it was full of carpets, and not uncomfortable.

It was on the southern slopes of Chigri that I came for the first time in this district upon the myrtle. The scarcity of this shrub shows how comparatively cold a winter climate this country has, considering its latitude. In Italy the myrtle grows in vast abundance, far to the north of Constantinople; but a Russian botanist whom I met at Therapia told me that he believed he had found on the Princes Islands, in the Sea of Marmora, the most northern station for the myrtle in this part of the world.

Winding round the base of Chigri through huge boulders of granite, from under which here and there peeped the lovely little cyclamen, we reached an ancient quarry, whence at some remote period —probably in the days of the Roman Empire—a number of huge columns had been hewn out of the granite. On this spot and on the road between it and the sea, there are, I think, eleven of these giants, 37 ft. high by 5 ft. 3 in. in diameter. No one knows who cut them or for what edifice they were designed. They reminded me of a scene in southern Sicily, where on the edge of one of the wild Palmetto wastes I once came upon some huge drums of columns that were evidently meant to form a part of the mighty temples of Selinus, lying there unused and unthought of some 2000 years after the temples of Selinus had been shaken into a mass of ruins.

From the granite quarries of Troas, an easy ride of about an hour and a half ought to have brought us to our night quarters, but a not-too-wise negro, who was one of our guides, and very confident in his local knowledge, led us astray, and we had some extremely rough scrambling before we and our horses extricated ourselves from the mountain defiles into which we had been taken, and came down upon a pleasant and level track, at the end of which rose the minarets of Ineh.

At Ineh our party divided, some returning to the farm, but I, with three others, riding over the plain in the direction of Ida to a place called Bairamitch. The journey, one of about twenty miles, was accomplished without adventures, and was not very interesting. At Bairamitch we were most hospitably received by the Governor, who lent me an admirable horse to replace a hired one which was a little overdone, and after three hours more we arrived at Evjilar, which lies by the banks of the Scamander on the slopes of Ida. The ride from Bairamitch was sufficiently picturesque, more especially two portions of it; first, where we crossed a branch of the Scamander, and looked up along its course shaded by beautiful oriental planes, and secondly, when the ex-robber chief, who had undertaken to make the necessary arrangements for our going up Ida, received us at Evjilar by torch-light.

We passed along, the Scamander flowing in a rapid stream between rocks on our left, and soon reached on the right the noble pines which, here replacing the cypress, shade the tombs of the rude forefathers of Evjilar, whilst straight in front stood "topmost Gargarus," looking worthy to be the seat of Zeus, as is told in the 14th book of the Iliad.

Our ex-robber chief, who, by the way, was careful to have us informed that his robberies had been dictated by very different motives from those which are so common in Greece, had provided us with some picturesque and armed barbarians as guides, rather than as escort (for Ida is just at present quite safe), and following their lead we were soon amongst the oak woods. * * * *

Very beautiful was the clearing of the mist off the mountain—

> "The swimming vapour sloped athwart the glen,
> Put forth an arm, and crept from pine to pine,
> And loitered slowly drawn."

Still, I should be giving you an entirely false impression if I led you to believe that Ida, as we saw it, or Ida, indeed, as it must be even in spring, is at all like the Ida of the Laureate's fancy. It is very beautiful, but it is as the real Yarrow was to the Yarrow which Wordsworth dreamt of.

More especially did I search in vain for any spot that appeared to me to be like the place where Paris was judge of the goddesses, although I am bound to say that one of our party could not subscribe to this view.

I never in travelling was reminded of that beautiful description except once, when turning sharply round a corner on Pentelicus, I came upon a spot where the white marble out of which so many incomparable statues had been carved, lay bathed in a perfect flood of the bright yellow Sternbergia. Then, indeed, the lines from Œnone flashed into my mind—

> "And at their feet the crocus brake like fire,
> Violet, amaracus, and asphodel,
> Lotus and lilies."

By the way, what has gained such a place in poetry for so very unpoetical a plant as the asphodel? The asphodel is the plant which used to be commoner than it is in our gardens under the name of the silver rod, and it covers great breadths of country in many of the classical regions, more especially in Sicily. I never could see that it had the twentieth part of the merit of unnumbered plants which grow in the same regions, and have been left unsung.

But I am digressing, and must return to the oak woods.

We soon found that great changes had taken place since our companion, to whose local knowledge we trusted, had been last upon the mountain, some fifteen years before. The whole management of the forests upon Ida had been altered. The old paths had disappeared, and for miles together there was nothing that could, by any stretch of courtesy, be called a track by any animal less given to climbing than a goat.

Long before we arrived at the spot, where under a mighty wall of limestone, the Scamander comes out of a dark and ice-cold cave, which has been explored for about 150 yards, but has still many secrets for the Alpine Club, we discovered that, so far from taking only five hours to go up and down, we should unquestionably take more than that even to go up. And so it proved, for considerably more than six hours had gone by, when having crossed the long belt of pines which extends from the end of the oak woods to the limit of arboreal vegetation, we entered upon the bare region of mica schist rock and loose stones, the so-called Phalacra of the Ancients, which forms the summit of Gargarus, itself the summit of the long range of Ida.

As we advanced I kept a sharp look-out for any flowers which the long-continued drought might have spared, and I have since had my scanty spoils carefully examined and identified at Kew. I found in blossom one specimen, and one only of a Digitalis—*the lævigata*, which must a few weeks before have been extremely conspicuous, for I passed many plants of it dried up and shrivelled. Higher up a crocus, perhaps *Speciosus*, was growing in great quantities, and higher still near the limit to which the pine ascends, a prickly Astragalus, which is, I am told, either the *Aristatus* or very near it, was found in much abundance.

The vegetation of the Phalacra is amusingly unlike that which Homer describes as having sprung up at the command of Zeus, in that locality. The Phalacra is as bare as the top of Ben Nevis, and not very unlike it. I found upon it a small Alsine, a dwarfed yellow Centaurea of the Cyanus group, and a small prickly Dianthus, which has been named for me *Erinaceus*, and which, growing in small hummocks, has a strong general resemblance to the Silene acaulis of our own higher mountains.

There was a good deal of haze in the distance, though the sky was cloudless; but the summit of Ida is too far distant from any place which was ever imagined to be Troy, for any one less keen sighted than the Ruler of Olympus himself, to see that city; let alone the movements of rival armies. I should guess it nearly forty miles, as the crow flies, from Hissarlik, and not much less from Bounarbashi.

On the northern side the view was *not* repaying, for we had seen all we saw, better from a lower elevation. The view on the southern side *was* repaying, for it was new to us and very beautiful. There at our feet half veiled by soft mist, was the Gulf of Adramyttium, and a small cluster of islands known as the Moschonisi, which lie on the landward side of the channel of Lesbos. There, too, was Lesbos itself, with just a peep of Scio in the remote distance. The range of Ida divides two widely differing climates. To the north of it the winds sweeping from Scythia prevent the growth of plants which are intolerant of frost, while to the south of it the lemon and many other trees flourish, which are not at home in the Troad. We were too far away to see details of this kind, but the appearance of the country was in harmony with the fact, and we could not forget that as we turned towards the north we were looking towards Thrace, and as we turned towards the south we were looking towards Ionia.

At length, turning our back on the tomb of the Turkish saint which now crowns the mountain of Zeus and Here, we descended by a route which was made much longer than it need have been by the necessity for keeping in the moonlight amid these rugged solitudes. And a glorious moonlight it was, searching out every recess of the mighty hills, and flooding crags and pinewood with its beams! * * * *

We were right glad to reach Evjilar, after an absence of just twelve hours. Here rest and some Turkish coffee soon revived us. We mounted our now fresh horses, and by midnight were back at Bairamitch, with its friendly Governor.

The next day dawned bright and cool; and, turning away from Ida, or rather from the great range of Ida—for I take it that nearly all the hills in these parts were loosely called Ida—we crossed the Scamander for the last time, and struck right north for the town of the Dardanelles. I have seldom enjoyed a more perfect ride upon a more perfect day. Soon after leaving Bairamitch we passed a little hamlet; and then for hours and hours our path lay among hills covered from foot to summit with that same exquisite light green pine which clothes the Isthmus of Corinth, and made the "fading garlands" of the Isthmian games. All this country is full of eagles. In fact, we saw more eagles than any other birds; and at one moment a member of our party counted no less than fifteen in sight.

At last, long after a moon as beautiful as that of the night before had lit us for some hours on our way, we got clear of the woody defiles through which we had been journeying, and came out upon the plains that lie between them and the sea. A ride of about one hour and a half or two hours brought us to the edge of the Hellespont; and we followed along its soft rippling waves till we forded the river Rhodius, and entered the town of the Dardanelles, in which every minaret had a chaplet of lamps in honour of the Sultan's birthday. * * * *

Next morning we embarked on board an Austrian Lloyd's steamer, and were soon out of the Hellespont.

We lay off Tenedos a long time, and had leisure to learn by heart the features of its wretched little capital, with its picturesque forts and long line of windmills; time, too, to wonder how a place that must always have been so insignificant had had the good luck to make such a noise in the world. Its most considerable product nowadays would appear to be wine—not that there is very much of that. Some, however, that we tasted was not to be despised. From Tenedos we ran along the coast, with distant views of Alexandria Troas, and of Chigri rising behind it, seeing, too, well the high lands across which St. Paul must have travelled when he walked across to Assos, sending the rest of his company round by sea. At length we reached Cape Baba, the ancient Lectum, the westernmost prolongation of the range of Ida; and, running close under it, with Lesbos, the island of Sappho, beautifully seen in front, we shaped our course to the eastward, as if we were going up the Gulf of Adramyttium.

Soon, however, just as the sun was setting, we turned to the south, and passed down the channel between Lesbos and the mainland, till we cast anchor in front of the town of Mytilene, which not only keeps its old Greek name, but has given that name to the whole Island of Lesbos.

A line drawn almost directly inland from a point on the shore opposite the place where we were lying would have struck Pergamus, which was, as you remember, one of the seven churches, and which before the Christian period had been the site of a brilliant monarchy, to which monarchy learning owes, if not the invention, at least the improvement, of parchment. A little further on is the mouth of the river Caicus, which separated Mysia from Æolia, and another place at which I would have given a good deal to have landed, because although it never has had its proper fame in the world, the time will, I trust, come when we shall raise statues in honour of its citizens. The place to which I allude is Cyme, and the reason why I hope we shall do so is this:—The inhabitants of Cyme were the fathers of free-trade. It is hardly possible to believe it, but we are positively assured that the inhabitants of this little town, in spite of the contempt that was poured upon them by the eleven other cities of Æolia, stuck steadily for 300 years to their determination not to raise any taxation on either imports or exports. Our own enlightened country, in the midst of what is called a system of perfect free trade, raises millions upon millions on her imports and exports.

When morning dawned we were running up the Gulf of Smyrna, and ere long the houses of the Levantine capital began to rise out of the water. We landed betimes, and were soon disembarrassed of any illusions which the aspect of the city, as seen from the sea, might have raised in our minds. Save that it is built upon a flat, Smyrna has every inconvenience of Constantinople, with no compensating advantages. The hotels are worse, the mosquitoes are more active, the streets are quite as dirty, and there is nothing like the same amount of colour or costume.

The line of railway, which is worked by an English company, and which runs from Smyrna to Aidin, the ancient Tralles, and the centre in these degenerate days of the fig-trade, is not very studious of the comfort of travellers in arranging its time-tables, and so it was necessary, in order to go and return from Ephesus in one day, that we should take a special train.

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Arrived at Ephesus, our first care was to explore the excavations on the site of the temple of Diana, which are now being made by English sappers and native workmen, under the superintendence of an architect of the name of Woods, who was the first to discover the site of this famous edifice. Antiquaries build many hopes upon his researches, and some interesting objects have already been sent hence to the British Museum.

Escaped from these unfinished, and as yet not very intelligible excavations, we proceeded to visit in their order all the more interesting points of Ephesus, which once spread far and wide over all the desolate hills and more desolate marshes amidst which we wandered; for Ephesus was a place of first-rate importance, one of those busy, seething, turbulent centres of human life of which there were not a few in the Roman world, and amongst which Alexandria stood preeminent.

One of the first places where we stopped is the cave which has been identified with the story of the seven sleepers, those Ephesian youths who, taking refuge with their dog from the persecution of Diocletian, slept for 200 years, and returned to Ephesus to find, to their utter astonishment, that the whole city had become Christian. The Mahometan religion has taken possession of this legend, and has, I am happy to say, accommodated the dog, whose name, it appears, was Ketmehr, with a place in Paradise, which, I have no doubt, like most of his kind, he richly deserves.

The next spot that detained us for any time was the theatre, attractive in itself, like all theatres of the ancient world, which form such a contrast to the bloodstained and hateful amphitheatres, but peculiarly interesting, from the fact that this was the scene of the tumult of Demetrius and the craftsmen, which is told so vividly in the Acts of the Apostles.

It lies on the edge of a hill, tier above tier of white marble seats, and must have looked as so many of these theatres did over the sea, which in old times came up far further into the plain of Ephesus than it does at present. * * * *

A short ride from this point brought us to the ruin, absurdly called the prison of St. Paul, but which well rewarded our climb by the view which it afforded of the sea with the sites of Colophon, one of the places which claimed to be the birthplace of Homer, Lebedos famous in the annals of the stage, and Teos the home of Anacreon, to be seen or guessed at in the distance. The brief October day was already done, when, after a long circuit amongst ruins and tombs, we returned to the station, and to Smyrna.

The line from Smyrna to Aidin runs south-east from Smyrna, but another English company has carried a line to the north-east.

By this line we started one morning, and, passing along the northern side of the beautiful Gulf of Smyrna, glided round the base of the volcanic range of Sipylus, and followed the valley of the Hermus to the exquisitely situated town of Manisa, the ancient Magnesia ad Sipylum, which lies between the river and the hills, surrounded by great cotton fields in which the people were busily engaged in picking the ripe bolls. From Manisa we skirted the base of Sipylus, passing the famous rock bas-relief of the Niobe, one of the oldest sculptural monuments of the Greek world.

At length we arrived at Cassaba, and mounting the horses that had been provided for us, rode off to the eastward. Our course lay along one of the great roads of the Turkish empire, and one of the oldest lines of communication in the world—the valley of the Hermus—up and down which there is a constant stream of traffic. Every few miles we met long strings of camels, or saw them resting by the side of their loads a little way to the right or left.

In front the plain extended for miles and miles, and several days of riding beyond our destination would hardly have brought us to the end of it. Still there was nothing monotonous in the effect, for on either side as well as behind stood up the most fascinatingly lovely mountains. The forms more especially of Sipylus, on which we looked back, and of Tmolus, which bounds the valley of the Hermus on the south, are a constant delight to the eye. At last, after some five hours' riding, we saw some scattered fragments of ruin, and forded a little stream which crossed our path, on its way to the Hermus. The little stream was no other than Pactolus of the Golden Sands, and the broken fragments of ruin were all that remained of the mighty Sardis, the seat of Crœsus and the Lydian kings, whose story fills half the first book of Herodotus.

We pitched our tents in the shadow of a ruin in a place where we ought, in accordance with all rule, to have caught fevers, which, however, more by good luck than by good guiding we did not do; and after visiting next morning the ruined Ionic temple of Cybele, the same which at a later period of Greek history—perhaps in the age of Alexander—replaced the temple, whose destruction by the Ionians led to the burning by the Persians of the temples of Greece, we crossed the Hermus, not far, I presume, from the spot where Cyrus overthrew Crœsus, and turned north to the extraordinary group of tumuli, called by the Turks the "thousand and one barrows," and which we know from Herodotus to be the tombs of the Lydian kings.

Our destination was the largest of these—the tomb of Alyattes, the father of Crœsus, the building of which is fully described by Herodotus, and which is compared by him to the Pyramids. This tumulus has been opened, and one of us penetrated into the very centre of it, to the infinite disgust of the bats. I preferred climbing to the top, from which I enjoyed a magnificent view over all the plain of the Hermus and the Lake of Gyges, which this huge tomb of Alyattes, being on the scale of a natural, not an artificial hill, had hitherto shrouded from the view. The district over which I was looking was probably the first spot known as Asia—a name which, spreading little by little, has now covered the whole of the vast continent that extends from the shore of the Egæan to the shores of the Pacific, unless, indeed, we are to seek the first Asia a little further to the south, close to Ephesus and the banks of the Cayster.

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After lingering some time amongst these tombs of the Lydian kings, of which Sir John Lubbock counted sixty in sight at once, we returned across the plain by another route in some few hours to Cassaba, were hospitably, and, indeed, sumptuously, entertained by the railway company, and were carried over the fifty-eight miles that separate Cassaba from Smyrna in two hours by a special train. This was the end of our brief sojourn in these interesting lands.

We embarked about two o'clock in the afternoon of the next day on board a large French steamer, which destiny having determined to make amends to us for the horrors of our middle passage from the Dardanelles, we had almost to ourselves. Before we had cleared the roads the Imbat, or daily sea-breeze, began to blow furiously, and the gusts that came down the gullies of the range of the "Two Brothers" upon our left gave promise of bad weather outside. The violence of the wind, however, began to moderate before we had reached the group of small islands close to the site of the ancient Clazomenæ, on which I looked with respect, because it has bequeathed to the world some of the most exquisite coins which have ever been struck, one of which by the way, a silver one forming part of a noble collection which was secured last summer by the British Museum, was valued at fully 200*l*. We moved steadily on, and ere evening were off the site of Phocaea, which we gazed upon with all the more interest because our destination was that far-off Marseilles, which had been founded by refugees from Phocaea when the generals of the Great King were ravaging Ionia. The sun set lurid and angry over Psara, the scene of one of the most terrible tragedies in the contest between the Greeks and the Turks, and the ancient Mount Mimas, the Kara Bournou or Black Cape of the Turks well deserved its name, as we, without any of those hesitations to which Nestor owns in the Third Book of the Odyssey, turned running close under it into the channel of Scio, where the Psariot Canaris performed the most remarkable exploit of the Greek War of Independence, by attaching a fire-ship to the gigantic vessel of the Capitan Pasha. When we were somewhere off Ritri, the ancient Erythræ, so called from the red colour of its rocks which contrast with those of the Black Cape to the northward, the wind became very high, and we found it so rough that we began to fear that the captain did not take too gloomy a view of the subject when he talked of being obliged next day to run for the safe harbour of Milo, an anticipation which happily was not realized. It gave, however, very appropriately, a further tinge of melancholy to our thoughts as we said good-by to Asia, and came to the end of a fortnight which was very agreeable to us, and which I wish I could deceive myself into thinking I had succeeded in making not altogether uninteresting to you.

[M. G. D.]

## WEEKLY EVENING MEETING,

Friday, May 16, 1873.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

PROFESSOR SIDNEY COLVIN, M.A.

### *On the Limits of Certainty in Taste or in Artistic Judgment.*

[Abstract deferred.]

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## WEEKLY EVENING MEETING,

Friday, May 23, 1873.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

WILLIAM SPOTTISWOODE, Esq. LL.D. M.A. Treas. R.S. Sec. R.I.

### *On Spectra of Polarized Light.*

SOME apology is perhaps due to you on my part for proposing that you should come together for such elementary experiments as those which I have to offer you this evening; but the growing importance of the subject of polarization, both in general and in astronomical physics, and the increased instrumental means at our command for the study and the exhibition of the phenomena, all seem to indicate that an hour may be not unprofitably spent in trying to obtain a clearer insight into the fundamental parts of the subject.

The object which I propose on this occasion is to verify experimentally the explanation furnished by the wave theory of the colours produced by crystal plates when submitted to polarized light. Not because the wave theory or its conclusions need in themselves any verification, but because the line of argument ought to be continuous throughout,—not like an electric current which leaps from one part of a broken conductor to another. Each line, whether theoretical or experimental, should be complete and continuous from one end to another.

Having this object in view, I propose to reproduce, by way of memorandum, a few of the phenomena in question, so as to bring them and their analysis in as close juxtaposition as possible; and for this purpose I have arranged two electric lamps, each furnished with a complete polarizing apparatus, but the second having in addition a pair of dispersive prisms, forming, in fact, a spectroscope. In the first arrangement I would draw your particular attention to a

Nicol's prism or polarizer, superior in respect of size, and in the opinion of those best capable of judging, not inferior in quality to any hitherto constructed. We have, in fact, before us, first the pair of Nicols constructed by Mr. Ladd, which accompanied Professor Tyndall in his journey to the United States; secondly, a larger one made for him by Mr. Browning; and thirdly, the largest of all, by Messrs. Tisley and Spiller. And lest it should seem that in thus proceeding from large to larger we have been aiming merely at indefinite size, I may explain that my object has been always a strictly definite one, *viz.* to obtain a polarizer large enough to transmit the entire beam which issues from the lantern; in this I have now completely succeeded.

Now it is well known that if a plate of selenite sufficiently thin be placed between two such Nicol's prisms, or, more technically speaking, between a polarizer and analyzer, colour will be produced. And the question proposed is, What is the nature of that colour? is it simply a pure colour of the spectrum, or is it compound, and if so, what are its component parts? The answer given by the wave theory is in brief this: In its passage through the selenite plate the rays have been so separated in the direction of their vibrations and in the velocity of their transmission, that, when re-compounded by means of the analyzer, they have in some instances neutralized one another. If this be the case, the fact ought to be visible when the beam emerging from the analyzer is dispersed by the spectroscope; for then we have the rays of all the different colours ranged side by side, and if any be wanting, their absence will be shown by the appearance of a dark band in their place in the spectrum. But not only so; the spectrum ought also to give an account of the other phenomena exhibited by the selenite when the analyzer is turned round, *viz.* that when the angle of turning amounts to 45° all trace of colour disappears; and also that when the angle amounts to 90° colour reappears, not, however, the original colour, but one complementary to it.

You now see in the spectrum of the reddish light produced by the selenite a broad but dark band in the blue; when the analyzer is turned round the band becomes less and less dark, until when the angle of turning amounts to 45° it has entirely disappeared. At this stage each part of the spectrum has its own proportional intensity, and the whole produces the colourless image seen without the spectroscope. Lastly, as the turning of the analyzer is continued, a dark band appears in the red, the part of the spectrum complementary to that occupied by the first band; and the darkness is most complete when the turning amounts to 90°. Thus we have from the spectroscope a complete account of what has taken place to produce the original colour and its changes.

It is further well known that the colour produced by a selenite, or other crystal plate is dependent upon the thickness of the plate. And, in fact, if a series of plates be taken, giving different colours, their spectra are found to show bands arranged in different positions. The thinner plates show bands in the parts of the spectrum nearest to the violet, where the waves are shorter, and consequently give rise to redder colours; while the thicker show bands nearer to the red, where the waves are longer, and consequently supply bluer tints.

When the thickness of the plate is continually increased, so that the colour produced has gone through the complete cycle of the spectrum, a further increase of thickness causes a reproduction of the colours in the same order; but it will be noticed that at each recurrence of the cycle the tints become paler, until when a number of cycles have been performed, and the thickness of the plate is considerable, all trace of colour is lost. Let us now take a series of plates, the first two of which, as you see, give colours; with the others which are successively of greater thickness the tints are so feeble that they can scarcely be distinguished. The spectrum of the first shows a single band; that of the second, two; showing that the second series of tints is not identical with the first, but that it is produced by the extinction of two colours from the components of white light. The spectra of the others show series of bands more and more numerous in proportion to the thickness of the plate, an array which may be increased indefinitely. The total light, then, of which the spectrum is deprived by the thicker plates is taken from a greater number of its parts; or, in other words, the light which still remains is distributed more and more evenly over the spectrum; and in the same proportion the sum total of it approaches more and more nearly to white light.

These experiments were made more than thirty years ago by the French philosophers, MM. Foucault and Fizeau.

If instead of selenite, Iceland spar, or other ordinary crystals, we use plates of quartz cut perpendicularly to the axis, and turn the analyzer round as before, the light, instead of exhibiting only one colour and its complementary with an intermediate stage in which colour is absent, changes continuously in tint; and the order of the colour depends partly upon the direction in which the analyzer is turned, and partly upon the character of the crystal, *i. e.* whether it is right-handed or left-handed. If we examine the spectrum in this case we find that the dark band never disappears, but marches from one end of the spectrum to another, or *vice versâ*, precisely in such a direction as to give rise to the tints seen by direct projection.

The kind of polarization effected by the quartz plates is called circular, while that effected by the other class of crystals is called plane, on account of the form of the vibrations executed by the molecules of æther; and this leads us to examine a little more closely the nature of the polarization of different parts of these spectra of polarized light.

Now, two things are clear: first, that if light be plane-polarized, that is, if all the vibrations throughout the entire ray are rectilinear and in one plane, they must in all their bearings have reference to a particular direction in space, so that they will be differently affected by different positions of the analyzer. Secondly, that if the vibrations be circular, they will be affected in precisely the same way (whatever that may be) in all positions of the analyzer. This statement merely recapitulates a fundamental point in polarization. In fact, plane-polarized light is alternately transmitted and extinguished by the analyzer as it is turned through 90°; while circularly-polarized light remains to all appearance unchanged. And if we examine carefully the spectrum of light which has passed through a selenite, or other ordinary crystal, we shall find that, commencing with two consecutive bands in position, the parts occupied by the bands and those midway between them are plane-polarized, for they become alternately dark and bright; while the intermediate parts, *i. e.* the parts at one-fourth of the distance from one band to the next remain permanently bright. These are, in fact, circularly polarized. But it would be incorrect to conclude from this experiment alone that such is really the case, because the same appearance would be seen if those parts were unpolarized, *i. e.* in the condition of ordinary lights. And on such a supposition we should conclude with equal justice that the parts on either side of the parts last mentioned (*e. g.* the parts separated by eighth parts of the interval between two bands) were partially polarized. But there is an instrument of very simple construction, called a "quarter-undulation plate," a plate usually of mica, whose thickness is an odd multiple of a quarter of a wave length, which enables us to discriminate between light unpolarized and circularly polarized. The exact mechanical effect produced upon the ray could hardly be explained in detail within our present limits of time; but suffice it for the present to say that when placed in a proper position, the plate transforms plane into circular and circular into plane polarization. That being so, the parts which were originally banded ought to remain bright, and those which originally remained bright ought to become banded during the rotation of the analyzer. The general effect to the eye will consequently be a general shifting of the bands through one-fourth of the space which separates each pair.

Circular polarization, like circular motion generally, may of course be of two kinds, which differ only in the direction of the motion. And, in fact, to convert the circular polarization produced by this plate from one of these kinds to the other (say from right-handed to left-handed, or *vice versâ*), we have only to turn the plate round through 90°. Conversely right-handed circular polarization will be changed by the plate into plane-polarization in one direction, while left-handed will be changed into plane at right angles to the first. Hence, if the plate be turned round through 90° we shall see that the bands are shifted in a direction opposite to that in which they were moved at first. In this therefore we have evidence not only that the polarization immediately on either side of a band is circular; but also that that immediately on the one side is right-handed, while that immediately on the other is left-handed.

If time permitted, I might enter still further into detail, and show that the polarization between the plane and the circular is elliptical, and even the positions of the longer and shorter axes and the direction of motion in each case. But sufficient has, perhaps, been said for our present purpose.

Before proceeding to the more varied forms of spectral bands, which I hope presently to bring under your notice, I should like to ask your attention for a few minutes to the peculiar phenomena exhibited when two plates of selenite giving complementary colours are used. The appearance of the spectrum varies with the relative position of the plates. If they are similarly placed, that is as if they were one plate of crystal, they will behave as a single plate, whose thickness is the sum of the thicknesses of each, and will produce double the number of bands which one alone would give; and when the analyzer is turned, the bands will disappear and re-appear in their complementary positions, as usual in the case of plane-polarization. If one of them be turned round through 45°, a single band will be seen at a particular position in the spectrum. This breaks into two, which recede from one another towards the red and violet ends respectively, or advance towards one another according to the direction in which the analyzer is turned. If the plate be turned through 45° in the opposite direction, the effects will be reversed. The darkness of the bands is, however, not equally complete during their whole passage. Lastly, if one of the plates be turned through 90°, no bands will be seen, and the spectrum will be alternately bright and dark, as if no plates were used, except only that the polarization is itself turned through 90°.

If a wedge-shaped crystal be used, the bands, instead of being straight, will cross the spectrum diagonally, the direction of the diagonal (dexter or sinister) being determined by the position of the thicker end of the wedge. If two similar wedges be used with their thickest ends together, they will act as a wedge whose angle and whose thickness is double of the first. If they be placed in the reverse position they will act as a flat plate, and the bands will again cross the spectrum in straight lines at right angles to its length.

If a concave plate be used the bands will dispose themselves in a fanlike arrangement, their divergence depending upon the distance of the slit from the centre of concavity.

If two quartz wedges, one of which has the optic axis parallel to the edge of the refractory angle, and the other perpendicular to it, but in one of the planes containing the angle (Babinet's Compensator), the appearances of the bands are very various.

The diagonal bands, beside sometimes doubling themselves as with ordinary wedges, sometimes combine so as to form longitudinal (instead of transverse) bands; and sometimes cross one another so as to form a diaper pattern with bright compartments in a dark framework, and *vice versâ*, according to the position of the plates.

The effects of different dispositions of the interposed crystals might be varied indefinitely; but enough has perhaps been said to show the delicacy of the method of spectrum analysis as applied to the examination of polarized light. [W. S.]

## WEEKLY EVENING MEETING,

Friday, May 30, 1873.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

The Earl of Rosse, D.C.L. F.R.S. M.R.I.

### *On the Radiation of Heat from the Moon, the Law of its Absorption by our Atmosphere, and its Variation in Amount with her Phases.*

In commencing his discourse, Lord Rosse remarked that of the three modes of action by light at a distance, which are believed always, to a greater or less extent, to exist together, one only—that of the direct action of the luminous rays on the eye—had been available to astronomers until very recent times; and he read a passage from Arago's works, in which he mentions that up to the time at which he wrote (1832)* no chemical action of the Moon's light on chloride of silver, nor heat effect on the most sensitive thermometer, had been detected in the concentrated rays of the Moon; also one from Sir J. Herschel's 'Outlines of Astronomy,'† to the same effect as regards absence of perceptible heat in the Moon's rays. He then described in order the successive attempts which have since been made to employ the thermopile where the thermometer had proved of inadequate sensitiveness. First, Melloni, having had placed at his disposal a segmental lens of 3 feet diameter and 3 feet focal length, after one or two unsuccessful attempts, was able to detect a heating effect due to the Moon's rays collected at its focus. Then about ten years later (Aug., 1856), Mr. Piazzi Smyth, while on Teneriffe, on directing his pile, armed with its polished conical reflector, alternately towards and away from the Moon, satisfied himself that the radiant heat was appreciable.

In 1861 Professor Tyndall experimented with a thermopile armed with a large tin conical reflector; but the smoke and heated air from the surrounding chimneys, which were rather less numerous in the direction of the Moon than in that to which he turned away his pile, completely masked, or rather overpowered, the heating effect from the Moon.

Professor Joule, in 1863, by means of a cylindrical glass vessel, divided in a vertical direction by a blackened pasteboard diaphragm, which extended to within one inch of the cover and of the bottom of the vessel, and in the upper of which spaces was delicately suspended a magnetized sewing needle furnished with a glass index, was able to detect the heat from a pint of water heated 30°, placed in a pan at 9 feet distance, also that of a moonbeam, admitted through an opening in a shutter, as it passed across the apparatus.

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* 'Annuaire du Bureau des Longitudes' for 1833.

† Page 285 (5th edition).

In the autumn of 1869 M. Marie-Davy, with a refractor of 9 inches aperture, detected the Moon's heat in the focus, and estimated the rise of temperature of the face of his thermopile at about twelve-millionths of a degree (Celsius); and a little earlier Mr. Huggins worked with the thermopile, but directed his attention more to the Fixed Stars than to the Moon.

During the course of the year 1868* an arrangement was prepared for measuring the heat in the image of the Moon, formed by the mirror of the 3-foot reflector at Parsonstown. For the purpose of further concentrating the heat of this image of 2·9 inches mean diameter on to the face of a thermopile ⅓rd of an inch diameter, a concave mirror of 8½ inches diameter and 3 inches focal length was employed; a rock-salt lens being objectionable from its condensing moisture on its surface, and, moreover, being hardly procurable of sufficient size.

To secure greater steadiness of the needle than was otherwise obtainable, a second similar concave mirror and thermopile were placed by the side of the first, the similar poles of the thermopiles being connected with each other, and the others with the terminals of the galvanometer. Thus the deviations due to the Moon's heating effect were proportional to the *sum* of the effects due to each pile separately, and those arising from disturbing causes, acting on the two piles, to the *difference* of the effects due to each pile. To secure still greater steadiness of the needle, the two piles of four pairs each, which, having been made at *different* times by Messrs. Elliott, were not of equal power, were replaced by two more equal thermopairs constructed on the spot.† The apparatus was enclosed on all sides, except on that towards the mirror of the telescope, with a box of tin and glass, and the lattice-tube was covered with a cloth to keep draughts of air from the piles. Two covered wires led from the thermopiles to the galvanometer in the observatory, and the heating effect was determined by directing the telescope so that the Moon's image fell alternately, for the space of one minute, on each of the two small concave mirrors.

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* See Proc. Royal Soc. 1869.

† In the construction of the thermopairs which were substituted for the thermopiles previously employed, the requisite sensitiveness was obtained by making the cross-section of the two elements small compared with the area of face, so that a smaller proportion of heat might be lost by conduction along the bars. On being compared with two of the ordinary construction they were found to be of about equal power, and therefore from the simplicity of construction they would appear to be deserving of the attention of physicists. (See Proc. Royal Soc. 1870.)

The observations made during the seasons 1868–9 and 1869–70, were found to follow pretty well Lambert's law for the variation of light with phase. It was found also that a piece of glass which transmitted 80 per cent. of the Sun's rays suffered only about 10 per cent. of the Moon's rays to pass through; thus a large amount of absorption before radiation from the Moon's surface was shown to take place.*

In the earlier experiments no attention had been paid to the correction to be applied for absorption of heat by the Earth's atmosphere; but, as the apparatus was gradually improved, it became indispensable to determine the amount of this correction before attempting to approach more nearly to the law of variation of the Moon's heat with her Phases than had been done in the earlier investigation.

By taking long series of readings for lunar heat through the greatest ranges of zenith distance available, a table expressing the law for decrease of heat with increase of zenith distance, closely following that deduced by Seidel for the corresponding decrease of the *light* of the stars, was obtained.† By the employment of this table, the determinations of the Moon's heat at various moments of the lunation were rendered comparable and available for laying down a more accurate "Phase-curve" than had been previously obtained.‡ This curve was found to agree more nearly with Professor Zöllner's law for the Moon's light, on the assumption that her surface acts as if it was *grooved* meridionally, the sides of the grooves being inclined at the uniform angle of 52° to the surface, than with Lambert's law for a perfectly *smooth* spherical surface.

To supply the want of the diagrams, which in the lecture exhi-

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* Assuming Wollaston's estimate of 800,000 : 1 to be the correct value of the proportion between sunlight and that of Full Moon, and the percentage of heat transmitted by glass to be 10 per cent. for the Moon's rays, and 80 per cent. for the Sun's (the difference being due to absorption before radiation from the Moon's surface), the corresponding ratio for solar and lunar heat deduced was 80,000 : 1. The later observations on lunar heat would require some modifications of this value, but in the present uncertainty as to the value for the proportion between sunlight and moonlight for which

| | | |
|---|---|---|
| Wollaston | gives the value .. .. .. .. .. | 801,072 |
| Bouguer | „ .. .. .. .. .. | 300,000 |
| Bond | „ .. .. .. .. .. | 470,980 |
| Zöllner, 1st method | „ .. .. .. .. .. | 618,000 |
| „ 2nd „ | „ .. .. .. .. .. | 619,000 |

further examination of the question would appear unprofitable.

† Following the reasoning from which Pouillet concluded that the Earth's atmosphere absorbs solar heat as if it were a medium of uniform density of height equal to $\frac{1}{80}$ of the Earth's radius, we deduced from the measurement of the Moon's heat, a height of $\frac{1}{80}$ of the earth's radius.

‡ In deducing the observations previous to employing them in the formation of a Phase-curve, corrections were applied for "Augmentation of the Moon's semidiameter," for change in the value of the Moon's distance, for reduction of the readings of the galvanometer from tangent to arc and variation of the Sun's distance. The last was not necessary in deducing the law of atmospheric absorption. Lambert's law for change with Phase was assumed in deducing the absorption law.

bited the laws of absorption in the atmosphere, and of variation of heat and light, with the phases, abbreviated tables are appended below.*

The distribution of light on two white globes, constructed in accordance with Lambert's and Zöllner's hypotheses, on which a beam from the electric light was thrown, was shown to be very different in the two cases; the brightest spot on the former being at the centre, and

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| Zenith Distance. | Light of Stars transmitted by Atmosphere. | Moon's Heat transmitted by Atmosphere. |
|---|---|---|
| 0° | 1·000 | 1·000 |
| 30 | 0·984 | 0·988 |
| 40 | 0·962 | 0·958 |
| 50 | 0·902 | 0·907 |
| 60 | 0·800 | 0·836 |
| 70 | 0·642 | 0·698 |
| 80 | 0·407 | 0·465 |
| 85 | 0·208 | — |

N.B.—Before entering the Atmosphere the Moon's heat = 1·262, so that at the Zenith fully ⅕th is absorbed before it reaches the Earth's surface.

| Distance from Full Moon. | Lambert's Formula. | Phase-curve for Heat (Observed). | Phase-curve for Heat transmitted by Glass. | Curve representing Zöllner's Photometric Observations. | Zöllner's Formula for Moon's Light. |
|---|---|---|---|---|---|
| 100° | 96 | 44 | — | — | — |
| 90 | 128 | 62 | — | — | — |
| 80 | 165 | 89 | — | — | — |
| 70 | 205 | 117 | 11·4 | 88 | — |
| 60 | 246 | 149 | 16·7 | 109 | — |
| 50 | 286 | 186 | 22·0 | 132 | 154 |
| 40 | 324 | 228 | 27·3 | 166 | 212 |
| 30 | 355 | 276 | 33·5 | 212 | 278 |
| 20 | 381 | 335 | 46·3 | 271 | 346 |
| 10 | 398 | 394 | 64·3 | 342 | 417 |
| 0 | 404 | 403 | 69·5 | 390 | 488 |
| 10 | 398 | 367 | 56·7 | 327 | 417 |
| 20 | 381 | 323 | 44·5 | 269 | 346 |
| 30 | 355 | 278 | 33·5 | 218 | 277 |
| 40 | 324 | 234 | 24·4 | 167 | 213 |
| 50 | 286 | 191 | 18·1 | 122 | 157 |
| 60 | 246 | 155 | 14·5 | 84 | 109 |
| 70 | 205 | 127 | 11·8 | 58 | 71 |
| 80 | 165 | 103 | 9·2 | 49 | — |
| 90 | 128 | 78 | 6·5 | — | — |
| 100 | 96 | 54 | 3·8 | — | — |
| I. | II. | III. | IV. | V. | VI. |

N.B.—To compare the heat transmitted by glass with Zöllner's photometric observations (Column V.) the quantities in Column IV. must be multiplied by 5·792.

on the latter at 52° on each side from the centre at the time of Full Moon, and at other times on the former at the bright limb, from which it gradually decreases towards the terminator; while on the latter there is a rapid decrease from the bright limb to a minimum about half-way to the terminator, after which it increases again, and then fades away on approaching the terminator.

On examining the Phase-curve which had been obtained, a certain want of symmetry on the two sides of Full Moon was perceived, which was ascribed to the unequal distribution of mountain and plain on the lunar surface, as was shown by a rough diagram of the lunar surface with its so-called "seas." It had also been found that the percentage of the Moon's heat transmitted by a sheet of glass diminished from 17·3 per cent. at Full Moon to about 13·3 per cent. at 22½°, 11 per cent. at 45°, and 10 per cent. at 67½° distance from Full Moon; a circumstance which might have been accounted for by supposing that there is a constant amount of radiant heat coming from the Moon in addition to that which, like the light, varies with the Phase, had it not been found that as the Moon approached tolerably near the Sun, as for instance, on March 27, 1871, when her distance from full was 138°, no perceptible amount of heat radiated from her surface.

The less rapid decrease of the Moon's heat than of her light on going farther from Full Moon, and the increase of percentage of heat transmitted by glass towards the time of Full Moon, may probably be explained on the assumption that when the Sun's heat and light strike the Moon's surface the whole of the former and only a certain proportion of the latter, depending on the intrinsic reflecting power or "Albedo" of the surface, leave it again, and consequently the shaded portions, which are inclined more towards the position of the Earth at Quadrature than at Full Moon, reflect a larger amount of heat as compared with the light at the former than at the latter time, and a greater flatness of the Heat- than of the Light- Phase-curve is the result.

With the view of obtaining a decisive result on the question, whether or not the Moon's surface requires an appreciable time to acquire the temperatures due to the various amounts of radiant heat falling on it at different moments, simultaneous determinations of the amount of the Moon's heat and of her light were made, whenever the state of the sky allowed of it, during the Eclipse of November 14, 1872. The Eclipse was a very partial one, only about $\frac{1}{40}$ of the Moon's diameter being in shadow; but although this circumstance, coupled with the uncertain state of the sky, rendered the observation far less satisfactory than it would otherwise have been, yet it was sufficient to show that the decline of light and heat as the penumbra came over the lunar surface and their increase after the middle of the Eclipse were sensibly proportional.*

Rather with the view of finding the value of the galvanometer readings in terms of the radiation from a surface of known temperature, and capable of being reproduced by anyone at any future time, than in the expectation of getting more than the roughest approximation to the temperature of the lunar surface, readings of the galvanometer were taken when the faces of the piles were alternately exposed for periods of one minute to the radiation from two blackened tin vessels filled with water of different temperatures. It was thus found that were the atmosphere removed, the surfaces of New and Full Moon might be respectively replaced by blackened tin vessels of equal apparent area to that of the Moon, and at temperatures of 50° and 247° Fahrenheit.*

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* Both were reduced to about one-half what they were before the eclipse.

In conclusion, Lord Rosse expressed a hope that among the many subjects which engage the attention of astronomers that of radiation of heat from the Moon might not be entirely lost sight of, more especially by those who live in a more favourable climate for such observations than that of the British Isles.

## WEEKLY EVENING MEETING,

Friday, May 16, 1873.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

### Professor Sidney Colvin, M.A.

### *On the Limits of Certainty in Taste, or in Artistic Judgment.*

[Extracts.]

The taste, or judgment, about which we are going to think, is that sensibility or sum of sensibilities within us which receives pleasure from the visible work of man's hands. I meditate no philosophical discussion of the nature of those sensibilities, nor any categoric survey of those classes of works which are designed to give them pleasure. Every hearer at all conversant with such inquiries would set down as an impertinence the attempt to do either within the limits of an hour's talking. On the other hand, it is safe to take for granted that, conversant or not, every hearer recognizes within himself the sort of powers which are meant when he is spoken to of his taste, or artistic judgment, and the classes of productions which he submits to it; and to that recognition, since I shall speak the language of no school, nor imply any other account of the matters in question than is in use in daily life, it is for the present purpose safe to trust.

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* In fact, assuming Dulong and Petit's law of radiation to hold, the change of radiation on going from New to Full Moon was the same as that produced by raising the temperature of the water through 197°, the initial temperature being 50° Fahr.

I suppose there is none of us to whom the sense of a perpetual difference of taste has not at some time or another given perplexity. If we care for beautiful things, or things we think beautiful, and care also for our neighbours, the two kindnesses become sometimes hard to reconcile. You are absorbed, in a picture gallery, before some work which appeals, it may be, to the strongest powers of admiration you have. You meet your friend, and he says, "What, you care about *that* thing!" or if he knows your susceptibilities, and has a soft heart, looks as much, and moves compassionately on. Turn to your newspapers, and we all know what kind of consent is to be found there. "A," writes one, "has eclipsed himself; who shall say but that great artist has set the seal of immortality upon his genius in his works, No. —." Of the same, "A," writes another, "has sunk even lower than we feared; no one, after us, will henceforth venture to claim merit for this ignominious pretender." That is the way in which we judge of contemporary appeals to our artistic sense; being conscious all the while that there ought to be some better agreement; and longing, as human nature will long in this as in weightier things, to be one with the majority of our kind; and that not with any ignoble majority of an hour, whom the next hour will silence, but with the high and elect majority whose voices, if we could hear them, are the spirit of the past and future. Turning to graver instances, we know how whole epochs of men have flouted the taste and the arts of other epochs, again to have their own flouted in their turn. We know how the eighteenth century flouted the arts and taste of the fifteenth century, and pitied the "hard manner of Perugino"; we know, too, how the nineteenth century began by turning round upon the eighteenth, and what we think of Pope's rhapsodies in honour of the dauber Jervas. Or, since we are getting upon individual instances, what can be more to the point than to think of the two men who, if we had to choose out of all modern Europe, ought assuredly to be chosen of these muses the priests—spirits, in Virgil's phrase,—smitten through and through with a mighty love, to be the ministers of their worship? The two men I mean are: for one, the German Winckelmann, who by passion as much as by learning penetrated a hundred years ago into the secrets of the Greek genius, before its best works were known, and is the father of the proper love and knowledge of antiquity in its monuments; and for the other, who but Mr. Ruskin. . . . . Winckelmann, speaking of architecture, says: "There are no good buildings of any kind in Florence; Venice is better, because of some of the palaces of Palladio." Mr. Ruskin, the child of our century, and enthusiast of the Middle Age, has taught us, we know what, about Santa Maria del Fiore and Giotto's campanile at Florence, and has observed, in a somewhat different spirit from Winckelmann's, we remember, on the palaces of Palladio as compared with other buildings in Venice. . . . .

That such instances of dissenting judgment should be, and threaten always to be, is a thing which subjects the lovers of art to varieties of troublesome questions from some who flatter themselves they have placed their affections more wisely. For example, the edifice in which I have the honour to address you is the home of the physical sciences, and this hall is the arena of some of the most brilliant and memorable of all demonstrations in science. Now assuredly many of the masters of science are also enthusiasts in art. But I have, probably we all have, friends distinguished in science who are rather disposed to be sceptical about art. In their own pursuits they are made familiar with exactness and certainty, and they are disposed to ask for exactness and certainty in all that can occupy the mind of rational beings. They expect that positiveness in judgment shall be the sign of certainty, and vehemence of exactness. They find the lovers of art positive indeed and vehement in their judgments; but not finding those judgments agree—sometimes, as we have seen, finding them even pretty strongly disagree—they ask for a right or wrong. They hear the professors of the subject loudly asserting a right and wrong; they listen, and find that two do not assert the same right or the same wrong; and then, perhaps not being very strongly drawn to the study by their own temperament and aptitudes, they pass on, and think slightly of it thereafter.

Obviously, with one slighting our study on these grounds, we might argue thus: You ask for certainty, we might say, but what kind of certainty? Do you, because your study is a study of the works of nature, and ours also a study of visible things having their existence, like the works of nature, in the material order of the universe, do you infer that it is a study like your own? Nay, the objects of our study (which besides are works of the human spirit, and not of nature) do not exist, like the objects of yours, to prove anything; they only exist to please. And when we are positive about right and wrong in the objects of our study, we do not assert a fact, but a feeling. Herein lies the initial difference. We both study visible objects in the first instance, and it is required for both that our powers of seeing and observing should be just and vivid. But there are two modes of seeing justly and vividly. Each sensation of sight has two properties: the property of awakening feeling within the mind, and the property of setting the mind to compare and judge. One mode of seeing makes most of that part in each sensation of sight which consists in feeling, and is connected with currents of pleasure in the mind; the other mode of seeing makes the most of that part of each sensation which consists in comparison and judgment, and is connected with the currents of reflection and calculation. Mine is the former mode of seeing, that which cannot see without beginning to enjoy; yours is the latter mode, that which cannot see without beginning to reason. And the difference so started runs right on. Your discriminative way of using your eyesight runs on, and leads you out beyond your most sustained processes of observation, into infinite chains of abstract ratiocination upon things observed. My enjoying way of using my eyesight runs on, and leads me, through my richest variety of pleasures in the mere colours and forms of things, into infinite combinations of pleasure imagined in association with the colours and forms. And so we have found our way into two distant kingdoms. Yours is the abstract kingdom of demonstration and deduction; mine is the concrete kingdom of delight and emotion. You are positive in the assertion of your facts, and with good reason. I am positive, perhaps reprehensibly, in the assertion of my feelings. Perhaps it is reprehensible; but it is the nature of mankind to be positive, to be even vehement, in asserting that which it feels strongly—to contend for the justice of its emotions even more than for the justice of its proofs. But pleasure, but emotion, even of the senses, still more of imagination and association, is evidently a thing much more personal than proof, and more variable for various constitutions. Therefore, when you hear the lovers of art contending positively and vehemently for the justice of their judgments, you must remember that their judgments are but the expression of their pleasures, and must not be surprised to find those pleasures not the same for them all, nor look among them for that kind of consent which a demonstration of your own is accustomed to command. Again, consider what complicated elements, in any of the higher kind of works of art, are offered for the artistic sense to pass judgment upon. In passing judgment upon any painting or sculpture, or work of art that imitates the objects of nature, the artistic sense has to think, not only of the play of the forms and lineaments there presented to it, but of originals represented behind those forms and lineaments; and of the relations the representations bear or do not bear to the models. If there is ample room for difference of feeling about the pleasurableness seen in actual presentation, how much more room for difference of it about things seen only in representation, called up only in idea, or in the idea of an idea behind the things actually seen!

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But, it may be said, is not Nature the standard? Is not a knowledge of Nature the one means for judging well of those arts which imitate her. Alas! but Nature, the conception of what is to be understood by that word, is so different for different people. In his famous seventh discourse, going over ground in part the same as that which I am attempting to traverse to-night, Reynolds divides the legitimate kinds of beauty, which may be a source of artistic pleasure, into two; one kind which is permanent and always the same, and which he identifies with Nature; another kind which is transitory and always changing, which he identifies with fashion or custom. And Nature, understood in that sense, Reynolds is never tired of setting up as the only standard. But nothing can be more emphatic than the way in which he distinguishes this permanent or standard Nature from another kind of nature which some understand by the word. The permanent and standard Nature, he says, is one and the same, which the consenting common-sense of mankind in all ages has established as such. To the consenting common-sense of mankind, and how that has fixed and interpreted the idea of Nature, you must look for your standard, and not to any particular or individual examples in actual nature. That is essentially an ideal conception; whereas the other conception of Nature, as a thing to be studied, imitated, and adhered to with scrupulous submission, in each one of her individual examples to which the artist has access, is an essentially real conception. It is the latter of the two which we have most insisted on in recent times. In insisting on the former of the two, the abstract or ideal conception, Reynolds, I think it is clear, was both right and wrong. He was right in feeling that Nature for the existing artistic sense, Nature in the sense which is almost synonymous with beauty, is something which the mind of man prefers, and follows and selects by preference, out of the multifarious shows of particular Nature, as point by point she is. He was confused with metaphysical notions when he held that Nature in this sense—that the chosen Nature, which is also Beauty—was one and the same thing all over the world. . . . .

This notion of the essential oneness of the beautiful, imported from metaphysics, where it is in its place, into life, where it is out of its place, has vitiated almost all theories of art, as here it vitiates Reynolds's. The fact is, that the artistic sense of man—that combination of sensibilities for pleasurable seeing, which leads him to prefer and pursue and take delight in some of the obscured points of Nature as she actually is, rather than others—the fact is, that this, in various races and ages of man, has preferred and pursued very different classes of observations. And the active and ingenious powers in the race and age being set on to produce according to the preference of the sensitive and enjoying ones, we have arts in which the modes of interpreting nature and feeling beauty vary very widely. No doubt you may collect all these and compare them, and find, in the world of facts, a general formula of excellence which shall include all kinds of beauty; and from that again you may proceed to deduct your canon. But this will not help you, often as it has been tried in the actual exercise of artistic judgment, to the attainment of exactness and certainty in it. Taste does not work—the artistic sense does not judge—by the application of canons from experience, any more than of formulas from metaphysics. Nay, one might conceive it possible for observation to collect, and for the sciences to analyze, any number of instances of those combinations which have been held beautiful in art. Bring the anatomy in a picture to the test of anatomic science, measure the waves of colour, find the formulæ of the curves, test the mountains by geology, and the animals by natural history: this is curious and interesting—for a work of art represents to the imagination things of which all these are the sciences; but it is not a help in judgment, or the action of taste.

How does taste work? It works by direct and intuitive decision upon the merits of each case. The whole sum of powers which coalesce within us to form a taste, or aptitude for artistic judgment, take cognizance of the result of the activities which have gone together to produce the work of art submitted to them. There are two constituent groups of powers which anyone will recognize within himself as taking part in his artistic judgments; the group which constitutes a sense of what the work of art on the surface and in itself is like, and the group which constitutes a knowledge of what it further means and tells. The sense is a sense of the art, and the knowledge a knowledge of the meaning; the one corresponds to what is sometimes called the form of a work of art, its play of lines, colours, and what things these at once suggest and represent to be right; the knowledge corresponds to what is sometimes called its matter or contents, the meaning and intention of the things represented—the ideas and feelings which the artist has desired to express—in a word, the phase of the human spirit of which his work is the embodiment.

Now, I should say that the only exactness, or certainty, of which artistic judgment properly so called is capable, is that which comes from the conjoint operation of these two groups of powers acting together in equal perfection. Perfect justice and keen susceptibility of the sense which apprehends and enjoys the form and appearance of a work of art, together with perfect knowledge of the circumstances and ideas that explain its matter and meaning. That is the only kind of certainty in such matters of which I have any knowledge. The one, the reverse of the former, must be perpetually stimulating and confirming the other, the knowledge of the matter, before either can be complete. Where both are complete, the two act inseparably together. It does not matter whether you call the state one of delighted intelligence or of intelligent delight; but your taste is as certain and exact as it can be.

Now, of these two constituent parts, the complex sense which apprehends in perfection the last subtleties of beauty, and the knowledge which informs upon the vital force and shades of meaning behind the beauty. I apprehend that the sense part can only be trained to perfection by what in plain words I shall call attention; and the knowledge part, like any other proficiency in knowledge, by what I shall call culture—attention for cultivating a taste for the beauty of art; culture for supplying knowledge about its meaning. That sounds as if it need not have taken so much saying; but, indeed, it comes to all I have to say.

If there were time, I should like to compare the kind of exactness in artistic judgment which I have put before you as my ideal, with other kinds of exactness, which might be understood or supposed. For instance, the expert judges exactly, whose life is given to distinguishing between an ancient gem and a beautiful *cinque-cento* forgery, or between one state of an engraving and another, or between two examples of the same. Yes, the expert's attainment is an admirable and enviable one; but unless he is an expert, and something more, he will not have certainty of taste in the high sense in which I understand it; because his sense and knowledge of both of them are of things accessory rather than essential; such as the mineral substance of a gem, the water-mark of paper in a print, the grain of canvas or kind of wood in a picture; and further, he is by profession more concerned to discriminate than to enjoy. There is the draughtsman's exactness, which is another matter; as it gives him precise knowledge of nature as she point by point is in her various appearances, and makes him familiar with the handicraft of whose results he is about to judge. If we were discussing the modes of exercising attention, the practice of draughtsmanship would evidently be among the first of them. But it would not be everything; since the perfected sense we require is precisely a sense of those subtleties of line and handling, infinitesimal to measurement, infinite in their effect upon feeling, which are beyond the reach of any draughtsmanship but such as first performed them; though not beyond the pursuit of the loving and attentive eye.

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The first results of a zealous and feeling practice of the attention—especially if it is carried on hand in hand with a search for the cognate kinds of knowledge—will be partly negative. If the admiration of the learner has lighted on good things at first, it will grow for those things immeasurably deeper and more fervent with his progress. But many of the things which he has admired or accepted he will give up. Not to mention the crude and measurable kinds of fault in the imitative parts of art, attention will reveal to him, and knowledge of circumstances will confirm, whole classes of qualities deserving his aversion. He will find that those forms of art which do not spring from real sensibility, but only from the demands of civilization that art should be, are hollow and offend. Such is the art of most periods of national exhaustion and decadence. Such, unfortunately, from quite other reasons, is too much of the ornament, a great deal of the architecture, not a little even of the imitative painting and sculpture of our own time. It is not the result of real or compulsive sensibilities at all, but of traditional demand. Then it becomes an effort and an affectation of the productive activities, and the properly-prepared receptive powers.

Here, then, as nearly as we can lay them down for common thought and daily use, seem to me to be the practical limits of consent—that is, of exactness or certainty—in these matters. It must be acknowledged that the certainty is neither of the logical nor scientific kind. It must be admitted that, though Metaphysic in her empyrean may think out the essential nature of the Beautiful never so wisely, and on her earth experience may never so justly build up canons of it, yet error is likely to come in when it is begun to apply either kind of principles to practical cases. The artistic judgment may, if it chooses, verify and vindicate itself by reference to principles either of the one kind or the other; but it does not and cannot act by the application of them. The artistic judgment is a judgment passed in each case on its merits by a vast and marvellously organized coalition of sensibilities and powers within the mind. To analyze that coalition down to its first constituents is the province of mental philosophy; to use, to wield it, is the prerogative of taste. We have spoken of the coalition roughly as having for two of its chief constituents a sense of beauty, or artistic sense, which judges of the visible form and immediate aspect of a work of art, or what it is like without inquiring further; and secondly, forms of culture, learning, or knowledge, which judge of its spiritual contents and signification, or what it further means and tells. Powers themselves, how complicated are these! and having to pass judgment on matters that seem how abstruse! And yet such is the nature of man that the two complex powers, by aptitude and exercise, may unite to act with the surest and most instantaneous facility. Such is the nature of art that the two obscures may combine into what—your complete work of art for its complete spectator—shall be one and as clear as day. The only legislation—the only jurisdiction is the proper word—the only jurisdiction that can regulate artistic judgments must come from those who to the natural endowment which makes them instinctive judges of the form—the artistic sense, the sense of beauty—have added the information, the knowledge and conversance in various phases of the human spirit, which enables them to understand its spiritual contents. The two endowments—the instinctive (so far as it is indeed instinctive) and the acquired—will tend to help and enrich the other. The knowledge of the meanings and spiritual contents of various works of art, which are the ideas behind the forms, will tend to enlarge and liberalize the natural sense of art's beauty and perfection, to guard it from prejudice or exclusive partiality, and teach it how unlike one beauty and one perfection may be to another. The sense of beauty will allure, with a summons of ever-growing potency, to the acquisition of knowledge and more knowledge yet, till the two, sense and knowledge, blend into each other as each is perfected, and the whole powers of the complete spectator take in at one wave of delighted apprehension—one wave with many currents—the whole work and genius of the complete artist.

Let this be the aim of all such as seek for certainty in taste. Let them, if they are conscious of the feeling for beauty, latent or developed, within them, cultivate it sedulously by attention, and above all be sure that it shall be always sincere and real. Let them inform, enlarge, and liberalize this instinctive sense with knowledge. The first result will be that of which I have spoken—a consent in intolerance, or certainty in condemning and eschewing what is essentially bad art. Show such an one what carries the marks of apathy beneath the affectation of feeling—he will none of it. Show him that which either on the side of form and the outward parts, is of essentially lax or incompetent performance; or else, on the side of matter and the spiritual parts, of essentially dull and cloddish conception—anathema! Show him what, assuming to be the work of art, is only the work of mechanism, however ingenious and finished, he will put it away.

But within these confines, round about which shall huddle and gibber those banished spectators of the false, the customary, and the mechanic — within them there shall be room for many shapes of various delight. As inexorable as your finely-organized and freely-informed spectator shall be for false art, so liberal shall he be in entertaining every various form of true. He will think it time that an end should be brought to those counter-currents and fluctuations of taste, onset, and recoil, which have prevailed for so many ages. He will desire no invidious exclusions, no oppositions of Greek and Gothic, Classic and Romance. Nay, culture has already got as far as that, and seen that there need be nothing irreconcilable between those who love best one or another of those two pre-eminent phases of the world's art. One man shall love best the arts of Greece for their high serenity and repose, their exquisite observance and combination of those elements of proportion and symmetry, which, from the simplest observance and combination may rise into the subtlest of our artistic pleasures, their realization in humane divinity of divine humanity, their self-centred strength, their clear completion, their rejection of all elements that entangle and perplex. Another shall love best—not quarrel with the first—the arts of mediæval Europe and of Italy, for their vexed aspiration struggling up, through abasement and perplexity, into peace still more perfect than the antique peace, or into a poignancy of rapture altogether new; for that quality of their beauty which comes of both a deeper plunge and a higher soar of thought than antiquity knew, carrying the impress of imagination enriched alike in joy and pain, and of hope entangled with despair. The lovers of these two, I say, need no longer quarrel as they have been disposed to do within our own memory. But there is a danger that either one of them, full (as who would not fain be full?) of the glory of what he delights in, will have no eye for lesser illuminations. The enthusiast of the great Greek or the great Italian phase of art is apt to despise the minor and derivative phases, intermediate or modern. That he ought not to do, any more than the enthusiast of Alpine scenery ought to despise the hop-gardens of Kent or the meadows of Ouse. He is apt to feel repelled from all alien or what he thinks barbaric phases of art, although in one of the primitive and strangest elements of pleasure—of colour—these phases are generally the foremost. For example, there is an alien—or call it a barbaric—phase of art which, in its way, many of us think a match for the Greek—I speak of the Japanese. A match for the Greek in types and representations of humanity?—no, indeed. In loftiness and serenity of the imagination, no; for the Japanese has an imagination more grotesquely intense and bedevilled than any Goth; but in the decorative parts, and not only of colour, but of the design, that and the parts that deal with plants and animals—yes. Look at the yelping and the pecking eagle side by side on the coins of Agrigentum, and then at any Japanese representation of eagles, and you will see that the two artists are akin. And so the more phases of such art you love, the better and more intelligently you are likely to love each of them. According to the spiritual antecedents and complexion of each spectator, his preferences will cling about one or another of the multifarious sorts of visible things, which the lands and ages that contained them for their pleasure have bequeathed to us. Let be, so long as each chooses some genuine end, and is not taken in with a vulgarity or a deception. One man in the modern world may have his face set to the past, and care for no art that does not represent it. Another may live only in his own time, and care for none representing anything but that. According as the ethical susceptibilities in one spectator are pure and tender, he will turn away from art that represents anything sullied or rude. According as another has those susceptibilities indifferent or refined away, or reserved for exercise in life only, he will not care what it is that art represents, so long as it represents that thing well and with a spirit. One imagination shall be all for the pathetic or squalid mysteries of Rembrandt; another, all for the frivolous sparkle of Watteau; another, all for the playful, thoughtful innocence and beloved grace of Reynolds. But none of these shall quarrel with the other, nor true culture with any of them. True culture shall rejoice, rather, that each has in his hand the key that fits it best, into an avenue, like none other for directness and delight, of communion with whatever the spirit of man in other ages and in our own has conceived with the most of joyfulness and freedom for performance with the most of love and skill.

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## GENERAL MONTHLY MEETING,

### Monday, June 2, 1873.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Miss Busk,
V. Carey-Elwes, Esq.
Charles W. Roberts, Esq.
The Hon. Edward Southwell Russell,
Col. William Henry Seymour, C.B.
James Taylor Smith, Esq.
John Berger Spence, Esq. F.G.S.

were *elected* Members of the Royal Institution.

With reference to the communication adopted at the preceding Meeting, the following letter from Lady Millicent Bence Jones to the Secretary was read:—

"84, Brook Street,
"*May* 7, 1873.

"Dear Mr. Spottiswoode,

"You have given his children and myself the greatest consolation that it is possible for one human being to impart to another in a sorrow such as ours; and next to the highest of all comfort is that of knowing that others are sorrowing with us, and bearing testimony to the excellence of the great and noble spirit that has left the world the poorer by his loss—a loss we must realize each day more fully.

"We are all most anxious to express our deep sense of the honour conferred upon my husband's memory, and the great kindness shown to those he has left, by the kind and most welcome message of sympathy which you have conveyed to us from the Managers and Members of the Royal Institution, and I must trust to you, in grateful return for the kindness, to express for us as you judge best the sincere thanks of the wife and children of him who has for so many years taken the deepest interest in all connected with the Royal Institution.

"It is a great support to me to meet with so much sympathy from those who were best able to appreciate his greatness, and all in him that makes his loss so deeply felt; and what you say of him comes home to my feelings in a way that words can poorly express.

"I can only thank you again for the comfort your letter has given me, and also for your kind wish to be of use to me, for which I feel grateful to you.

"Ever gratefully yours,
"M. Bence Jones."

The special thanks of the Members were returned to Sir Henry Holland, Bart. the President, for his Fifteenth Annual Donation of Forty Pounds.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

From

*Actuaries, Institute of*—Journal, No. 91. 8vo. 1873.
*Astronomical Society, Royal*—Monthly Notices, Vol. XXXIII. No. 6. 8vo. 1873.
*Bertier, Dr. F. (the Author)*—Des Eaux Minérales de la Savoie. (L 16) 8vo. 1873.
*British Architects, Royal Institute of*—Sessional Papers, 1872-3, No. 11. 4to.
*British Museum Trustees*—Catalogue of Hemiptera Heteroptera, Part 7. 8vo. 1873.
  Catalogue of Syriac MSS. Part 3. 4to. 1872.
  Handbook of Shield Reptiles. 8vo. 1873.
*Chemical Society*—Journal for May, 1873. 8vo.
*Editors*—American Journal of Science for May, 1873. 8vo.
  Athenæum for May, 1873. 4to.
  Chemical News for May, 1873. 4to.
  Engineer for May, 1873.
  Food Journal for May, 1873. 4to.
  Journal for Applied Science for May, 1873. Folio.
  Journal of Gas-Lighting for May, 1873. 4to.
  Nature for May, 1873. 4to.
  Nautical Magazine for May, 1873. 8vo.
  Once a Week for May, 1873. 8vo.
  Pharmaceutical Journal for May, 1873. 8vo.
  Telegraph Journal, No. 7. 8vo. 1873.
*Ella, Professor (the Author)*—Lectures on Dramatic Music. (M 9) 4to. 1872.
*Elliot, John Lettsom, Esq. M.R.I.*—The Life of Lloyd, first Lord Kenyon, Lord Chief Justice of England. By the Hon. Geo. T. Kenyon. 8vo. 1873.

*Franklin Institute*—Journal, No. 568. 8vo. 1873.
*Geological Society*—Quarterly Journal, No. 114. 8vo. 1873.
*Liverpool Literary and Philosophical Society*—Proceedings, No. 26.
Index, Vols. 1–25. 8vo. 1871–2.
*Meteorological Society*—Quarterly Journal, No. 6. 8vo. 1873.
*Photographic Society*—Journal, Nos. 240, 241. 8vo. 1872.
*Preussische Akademie der Wissenschaften*—Monatsberichte, Jan. 1873. 8vo.
*Roma, Reale Accademia dei Lincei*—Atti, Tomo XXV. Sess. 7. fol. 1873.
*Roscoe, Professor H. E., F.R.S. &c. (the Author)*—Spectrum Analysis. 3rd Edition. 8vo. 1873.
*Royal Society of Tasmania*—Monthly Notices of Proceedings. 1871. 8vo.
Results of Meteorological Observations for Hobart Town. 4to. 1872.
*Royal University of Norway*—Various Memoirs. 4to. 1872–3.
Norske Meteorologisk Aarbog. 1871. 4to.
G. O. Sars: On Animal Life from the Great Deeps off Norway. 4to. 1872.
S. A. Sexe: On the Rise of Land in Scandinavia. (M 9) 4to. 1872.
*Seaman, Ezra C. Esq. (the Author)*—Views of Nature. 12mo. 1873.
*Statistical Society*—Journal, Vol. XXXVI. Part 1. 8vo. 1873.
*Symons, G. J. Esq. (the Author)*—Symons' Monthly Meteorological Magazine, May, 1873. 8vo.
*United Service Institution, Royal*—Journal, No. 71. 8vo. 1873.
*Wild, H. (the Director)*—Annalen des Physikalischen Central-Observatoriums. 1871. 4to. St. Petersburg. 1873.
*Yorkshire Philosophical Society*—Communications, 1872. 8vo. 1873.

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## WEEKLY EVENING MEETING,

### Friday, June 6, 1873.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

### Professor Odling, M.A. F.R.S.

### *On Evaporation and Diffusion.*

Of the circumstances on which the rate of evaporation of the same volatile substance chiefly depends, one very influential circumstance, namely, the nature of the atmosphere into which the vapour proceeds and distributes itself, does not appear to have been made the subject of careful examination. There is, however, one familiar experiment on the subject; and it consists in passing up a little ether into two equal volumes, one of hydrogen and the other of air, contained in similar cylinders standing over water. Evaporation of the ether takes place in both cases; though far more rapidly into the hydrogen than into the air, as shown by the far more rapid expansion of the hydrogenous than of the aerial space, from the addition to it of the ether-vapour. This experiment is usually cited in illustration of the greater rapidity at which ether-vapour diffuses itself into an atmosphere of light hydrogen than into an atmosphere of comparatively heavy air. It is doubtful, however, whether the explanation in this precise form is strictly correct, the diffusive mobility of any gas or vapour being a property altogether special to itself, irrespective of extraneous conditions. Thus, in a particular experiment, Mr. Graham found that while the quantity of hydrogen which in one minute of time passed through a graphite plate into a vacuum was 1·289 c. c., the quantity of hydrogen which in the same time passed through the same graphite plate into the surrounding air was 1·243 c. c., or almost exactly the same.

A novel experiment on the influence due to the nature of the contiguous atmosphere upon the rapidity of the process of evaporation, consists in introducing into each of two similar tall cylinder bottles a sealed glass bulb containing a little iodine. The one cylinder being filled with hydrogen, and the other with air, and each having suspended from its mouth a piece of starched paper or cloth, the bulbs are then broken by concussion of the containing cylinders. But, in this case, the test paper or cloth suspended in the cylinder of air becomes affected by the iodine considerably before that suspended in the cylinder of hydrogen; or, contrary to what happens with ether, the iodine *appears* to volatilize more quickly in the comparatively heavy air than in the much lighter hydrogen. Similarly, if some weak aqueous ammonia contained in a sealed glass bulb be set free in a tall bottle of hydrogen, and also in a tall bottle of air, a piece of turmeric paper suspended from the stopper of each bottle becomes affected by the ammonia vapour more quickly in the bottle of air than in the bottle of hydrogen.

Water may be taken as the type of a volatile substance, and the nature of the process of evaporation be conveniently studied in relation thereto. Now, in a hermetically-closed vessel exhausted of air, but partly filled with water, the matter of the water exists in two states—a portion of it in the liquid state, and another portion in the aerial, or vaporous, or evaporated state; and, with excess of liquid water present, the quantity of vaporous water that can exist in an otherwise vacuous space, or the density of the vapour filling the space, is found to vary solely with the temperature. Thus 100 cubic inches of space will contain at 58° C., 3 grains of vaporous water; at 100° C., 15 grains; at 144° C., 58 grains, and so on; or the densities of the vapour of water at these different temperatures are as the numbers, 3, 15, and 58, respectively. Now, the quantity of water-vapour that can exist at any particular temperature in a volume of otherwise vacuous space, is almost identical—it used to be considered absolutely identical,—with the quantity of vapour that can exist in the same volume of space already occupied by air. In other words, a cubic inch of water-vapour, and a cubic inch of air saturated with water-vapour, contain the same weight of the matter of water. Hence it is found that with continuous increase of temperature, a greater and greater quantity of liquid water ceases to be liquid and becomes vaporous, or is, in other words, evaporated into the adjoining space, whether vacuous or aerial; and conversely with decrease of temperature, a greater and greater quantity of vaporous water ceases to be vaporous, and is deposited in the liquid state. Thus, upon cooling a globe of transparent warm moist air, it is at once filled with an opaque mist, which, left to itself, soon becomes deposited in the form of drops; but which may be readily made to disappear or re-vaporize by gently heating the globe. The deposition of dew on cold surfaces brought into a warm moist room is familiar to all.

The tension or spring of water-vapour varies with the quantity of vapour filling the space—that is, with the density of the vapour, and consequently with its temperature. Thus the tension is equal at 15° C., to a pressure of 13 millimètres of mercury; at 60° C., to a pressure of 149 millimètres; at 100° C., to a pressure of 760 millimètres; at 144° C., to a pressure of 3040 millimètres. At the temperature of 100° C., the tension of the vapour of water being equal to a pressure of 760 millimètres of mercury, balances the tension or pressure of the atmosphere, as measured by the height of the barometric column. Hence on continuing to heat water already at the temperature of 100°, it boils, or is converted into vapour, having a tension equal to that of the atmosphere. And so with a less or greater external pressure, water boils at a lower or higher temperature—in each case at the particular temperature whereat the elastic force of the water-vapour balances the particular external pressure. Under a pressure of 149 millimètres, for instance, water boils at 60° C.; under a pressure of 3040 millimètres, it boils at 144° C. The occurrence, then, of ebullition at some particular point of temperature may be viewed as a mere accident of the process of evaporation—a more and more rapid evaporation, with production of vapour of higher and higher density and elasticity, proceeding continuously with the continuous increase of temperature, although, indeed, at a far more rapid rate than the rate of increase of temperature.

In the conversion of liquid water into water-vapour, heat is absorbed. It was at one time considered that the quantity of heat absorbed was independent of the temperature and correlative tension of the vapour yielded, and that it varied only with the quantity yielded. It was held, for example, that in the conversion, say, of a gramme of liquid water into a gramme of water-vapour—whether of tenuous vapour at a low temperature, or of dense vapour at a high temperature—the same quantity of heat was absorbed. This rule, though now known to be only approximatively true, is nevertheless sufficiently true to allow of the amount of cold, or absorption of heat, resulting from any evaporation, being taken as an indication or measure of the amount of that evaporation. The depression of temperature caused by the evaporation of water, is familiarly shown in the use of the wet-bulb hygrometer. This instrument consisting simply of a couple of thermometers, one of which has its bulb kept continuously moist, the difference in way of deficit between the temperature manifested by this wet-bulb thermometer and the temperature manifested by the other thermometer, is a measure of the amount of water evaporating from the surface of the moistened bulb.

What is true of liquid water and of water-vapour is true, in principle, of other volatile liquids and of their several vapours. At any given temperature some of these bodies volatilize much more rapidly, others of them much less rapidly, than one another, and than water; and in the process of their evaporation some of them absorb a much greater amount of heat than others, though for the same weight of vapour formed no one of them is known to absorb so large an amount of heat as water. Disulphide of carbon, ether, and alcohol, are familiar examples of bodies more volatile than water; aniline, mercury, and molten silver, of bodies less volatile. It is well known that water when required pure is distilled—that is to say, it is first vaporized by heat and then re-liquefied by cold; and silver when required absolutely pure is sometimes submitted to the same process of distillation. Evaporation is, moreover, a property not only of liquid, but also of solid bodies, as of sal-ammoniac, iodine, camphor, ice and snow, solid carbonic acid, &c. Many of the phenomena of evaporation can be illustrated more conveniently with other volatile substances than with water. Thus the cooling produced by evaporation may be strikingly illustrated by means of ether, and still more strikingly by means of some liquefied gas, as liquefied sulphurous or nitrous oxide.

The necessary influence of some conditions on the rate of evaporation is obvious. Evaporation being a direct result of the absorption of heat, and the higher the temperature the greater the quantity of vapour capable of existing in a given space, it is found that at higher and higher temperatures water evaporates or dries up more and more rapidly, in proportion approximatively to the increasing density of the vapour at higher and higher temperatures. Wet linen, for example, is habitually dried by being hung before the fire or placed in heated chambers or ovens.

Similarly with regard to the influence of the extent of liquid surface on the rate of evaporation, it is found that the evaporation of a given weight of liquid water takes place more rapidly when the water is spread out in a wide dish than when it is contained in a narrow tube, in proportion to the greater extent of surface from which the evaporation can take place in the one case than in the other. Thus, while half a pint of water in a tumbler will remain for an almost indefinite time without becoming dried up, half a pint of water absorbed in a thick towel exposing on its two sides more than a dozen square feet of surface, will become dried up in the course of twenty-four hours.

Again, the influence exerted by the greater or less dryness of the adjacent space, whether or not aerial, upon the evaporation of water is obvious. Into a space already saturated with vapour no further evaporation whatever can take place; and other conditions being alike, the drier the space the greater the amount of evaporation that can take place with it, and the more rapid the process of evaporation.

In the laboratory, moist substances are made dry by allowing the water from them to evaporate into spaces kept artificially dry by means of desiccating agents, such as oil of vitriol, quicklime, chloride of calcium, &c., which absorb the water-vapour from the space as fast as it is produced therein. Moreover, evaporation proceeding into any space, vacuous or aerial, until the space is saturated with water-vapour, and more rapidly in proportion to the removal from saturation of the space, it is obvious that, other conditions being equal, evaporation proceeds more rapidly into a large space requiring for its saturation a considerable weight of water-vapour than into a small space capable of being saturated by a minute quantity of the vapour.

The influence of the extent of aerial space into which a vapour can distribute itself upon the rate of formation of the vapour, leads to a consideration of the effect of the movement of the contiguous air upon the rate of evaporation from a given surface. With an absolutely quiescent atmosphere, the layer of atmosphere in immediate contact with the surface of evaporating liquid must become quickly saturated with vapour; and but for some spontaneous means of removal of the vapour itself, or of the air saturated with it, from the surface of the liquid, all further evaporation would be arrested. The nature of these spontaneous means of removal will be considered presently; but the influence of the artificial removal of the more or less saturated air from the surface of the evaporating liquid can be easily manifested. Thus on allowing a strong current of air to play upon a surface of ether contained in a thin glass vessel standing on a stratum of water, the stratum of water becomes quickly frozen by reason of the heat abstracted from it, in the rapid production of ether-vapour which takes place in the current of air. For whereas with the ether in the vessel left to itself, the interior of the vessel soon becomes saturated with ether-vapour, and further evaporation with correlative cooling effect is almost arrested; with a current of air blown on to the surface of the ether, the liquid ether is in continuous contact with a fresh unsaturated atmosphere, into which it continues to evaporate with rapidity.

In connection with the above influences, it is observable that the evaporation of water from growing plants is favoured by the three conditions of extent of leaf surface, movement of the more or less dry atmosphere, and heat of the summer sun. An early and very striking experiment on the extent of this evaporation was made by Dr. Stephen Hales in 1724, and is recorded in his 'Vegetable Statics.' He found that with a healthy full-grown sunflower more than a yard high, planted when young in a suitable pot, the evaporation from the plant itself amounted on the average to 20 ounces in a twelve hours' day, the maximum quantity being 30 ounces. Some very interesting and exact modern experiments on this subject were conducted in 1850 by Mr. Lawes at Rothamstead. As an example of the results obtained, he found that a plant of barley, during a period of 172 days' growth, in which it acquired 419 grains of dry organic, and 46 grains of dry mineral matter, evaporated not less than 120,000 grains, or upwards of 17 lbs. of water; so that for every grain of dry matter, organic and mineral, fixed by the plant during its period of growth, 257 grains of water were absorbed and evaporated.

A most important condition affecting the rate of evaporation is the pressure on the surface of the volatile substance. Into a vacuum indeed, where there is no pressure, evaporation is practically instantaneous. Thus on letting up a little water, alcohol, or ether into the vacuum of a barometer, the mercury is at once depressed to the maximum extent producible by the tension of the particular vapour at the particular temperature. Again, on breaking a sealed bulb of iodine, within and at the lower end of a long sealed vacuous tube, a piece of starch-paper at the upper end of the tube is almost immediately affected by the iodine vapour. Further, the cooling consequent on the rapid evaporation of ether, ammonia, &c., into a vacuum, is taken advantage of in the construction of several well-known freezing machines. Again, in Leslie's celebrated experiment, the evaporation even of water into a vacuous space, from which the water-vapour is removed by some desiccant as fast as produced, takes place with such rapidity and correlative absorption of heat, as to effect the freezing of the residual water from which the evaporation is proceeding. The same result is effected in Wollaston's cryophorus, the water-vapour in this case not being absorbed by oil of vitriol or other desiccant, but being simply condensed by cold applied to the other and distant bulb of the twice bent vacuous tube. Moreover, it was long ago shown by Daniel that evaporation of water into the same dry atmosphere takes place at a rate inversely proportioned to the pressure of the atmosphere. Thus in a particular set of experiments, the pressures of the air being 30·4, 15·2, and 7·6 inches of mercury, the quantities of water evaporated in half an hour were found to be 1·24, 2·97, and 5·68 grains respectively.

That the nature of the atmosphere into which a vapour can distribute itself has an important influence on the rate of formation of the vapour may be manifested by several experiments, in addition to those shown at the beginning of the lecture. Thus with a couple of thin glasses standing on a stratum of water, and containing each some rectified wood-spirit, if the one portion of spirit be blown upon by a strong current of hydrogen, and the other portion by a strong current of air, the stratum of water underlying the glass of spirit blown upon by the hydrogen, will alone become frozen, by reason of the excessive vaporization of, and heat-absorption by, the spirit in contact with the atmosphere of hydrogen. Again, a series of experiments has recently been made by the speaker on the relative rates of evaporation of water into limited atmospheres of hydrogen, air, and carbonic acid respectively, alike kept dry by contact with a large surface of oil of vitriol. In these experiments, the evaporation was found to take place most rapidly in hydrogen, and least rapidly in carbonic acid; and as a mean of seven tolerably concordant results, the ratio of the rate of evaporation in hydrogen to the rate of evaporation in air, under the particular conditions of the experiment, was found to be as 2·68 : 1.

The influence of the particular contiguous atmosphere on the rate of evaporation of any particular liquid may be, and probably is, of a very complex nature. There are, however, two easily intelligible ways in which the particular atmosphere may act by virtue of its specific gravity. Thus a lighter or heavier gas or vapour ascends or descends bodily through another at a rate proportionate to the difference in their specific gravities, a very slight difference sufficing to bring about an upward or downward current of considerable activity. This upward or downward movement is well shown by introducing first heavy ether-vapour, and then light coal-gas, through a lateral opening made at about the middle of the length of a tall somewhat wide perpendicular glass tube, open at both ends; when it will be found that the ether-vapour, descending by reason of its heaviness, can only be inflamed at the bottom of the tube, while the coal-gas, ascending by reason of its lightness, can only be inflamed at the top. Now, the specific gravities of hydrogen, aqueous vapour, and air at the same temperature, being to one another as the numbers 1, 9, and 14·5 respectively, it is obvious that whereas hydrogen saturated with water-vapour is heavier than dry hydrogen, air saturated with water-vapour is lighter than dry air. Accordingly, with a surface of water exposed to an atmosphere of originally dry hydrogen, the hydrogen in actual contact with the water will not, by becoming moist, acquire a tendency to rise up through the dry hydrogen, and so be removed from the surface of the liquid; whereas, with a surface of water exposed to an atmosphere of originally dry air, the air in actual contact with the water will, by becoming moist, acquire a tendency to rise up through the dry air, and so become removed from the surface of the liquid. And having regard to this influence alone, water when occupying an inferior position, should evaporate more rapidly into air than into hydrogen. It must be borne in mind that any given volume of moist air or gas is the sum of the volume of its constituent water-vapour, and of the volume of its constituent dry air or gas; and that the tension of the moist air or gas is the sum of the tension of its constituent water-vapour, and of the tension of its constituent dry air or gas, actually or virtually expanded by the addition to it of a certain volume of water-vapour. So that with water boiling in a deep open vessel, there may exist in close proximity to each other, almost pure water-vapour, different mixtures of water-vapour and air, and comparatively dry air, all having alike a tension of 760 millimètres of mercury.

But independently of their bodily movements in the form of currents, gases and vapours distribute themselves among one another by a proper molecular movement of diffusion—the relative diffusive mobilities of different gases or vapours being inversely as the square roots of their several specific gravities. Accordingly, with equal surfaces of water exposed respectively to an atmosphere of hydrogen and to an atmosphere of air, the vapour produced from the surface of the water is interpenetrated and distributed through the contiguous space with greater rapidity by the highly-diffusive hydrogen than by the feebly-diffusive air, in proportion to their relative diffusion-velocities, 3·8 and 1·0 respectively. And it is, doubtless, to the more rapid diffusion of hydrogen than of air into the vapour as formed, and the consequent more rapid supply to the liquid of an unsaturated atmosphere into which it can evaporate, that the more rapid evaporation of water, ether, and wood-spirit into an atmosphere of hydrogen than into an atmosphere of air, as in the experiments, for instance, already shown and described, is substantially due.

[W. O.]

## GENERAL MONTHLY MEETING,

Monday, July 7, 1873.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. President, in the Chair.

Colonel Charles K. Bushe,
Matthew Greene, Esq.
Walter Raleigh Trevelyan, Esq.

were *elected* Members of the Royal Institution.

The Secretary having reported that Dr. Odling had resigned the Fullerian Professorship of Chemistry on July 3rd,

Resolved, That the best thanks of the Meeting be given to Dr. Odling for his efficient services as a Professor of the Institution, and that the Meeting regret on behalf of the Institution the loss of his future services.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

From

*Alcock, Col. M.R.I. (the Author)*—The Relative Power of Nations. 8vo. 1873.
*American Academy of Arts and Sciences*—Memoirs of Count Rumford. By G. E. Ellis. 8vo. 1871.
*American Academy of Natural Sciences, Philadelphia*—Proceedings, 1872. 8vo.
*American Philosophical Society*—Proceedings, No. 89. 8vo. 1873.
*Asiatic Society of Bengal*—Journal, Part 1, 1872, Nos. 2, 3; 1872, Part 2, No. 4. 8vo.
Proceedings, 1872, No. 10; 1873, No. 1.
*Astronomical Society, Royal*—Monthly Notices, Vol. XXXIII. No. 7. 8vo. 1873.
*British Architects, Royal Institute of*—Sessional Papers, 1872–3, No. 12. 4to.
*Chemical Society*—Journal for June, 1873. 8vo.
*Cornwall Polytechnic Society, Royal*—Fortieth Annual Report. 1872. 8vo.

*Cox, E. T. Esq. (the Author)*—Third and Fourth Reports of the Geological Survey of Indiana. 2 vols. 8vo. 1872.
*Editors*—American Journal of Science for June, 1873. 8vo.
Athenæum for June, 1873. 4to.
Chemical News for June, 1873. 4to.
Engineer for June, 1873.
Food Journal for June, 1873. 4to.
Journal for Applied Science for June, 1873. Folio.
Journal of Gas-Lighting for June, 1873. 4to.
Nature for June, 1873. 4to.
Nautical Magazine for June, 1873. 8vo.
Once a Week for June, 1873. 8vo.
Pharmaceutical Journal for June, 1873. 8vo.
Telegraph Journal, No. 9. 8vo. 1873.
*Franklin Institute*—Journal, No. 569. 8vo. 1873.
*Geographical Society, Royal*—Proceedings, 1873, No. 2. 8vo.
*Glasgow Philosophical Society*—Proceedings, Vol. VIII. No. 2. 8vo. 1873.
*Horticultural Society, Royal*—Journal, New Series, Vol. IV. Part 13. 8vo. 1873.
*Linnean Society*—Journal, Nos. 70–72. 8vo. 1873.
*Mayer, Professor A. M. (the Author)*—Lecture Notes on Physics. 8vo. 1868.
Various Papers. 8vo. 1869-73.
The Earth a Great Magnet. (O 16) 16to. 1872.
*Melbourne Industrial Museum*—Lectures. 8vo. 1872.
*Photographic Society*—Journal, No. 242. 8vo. 1872.
*Preussische Akademie der Wissenschaften*—Monatsberichte, Feb. 1873. 8vo.
*St. Pétersbourg, Académie Impériale des Sciences*—Mémoires, 7$^e$ Série, Tome XVIII. Nos. 8–10; Tome XIX. Nos. 1, 2. 4to. 1872.
Bulletins, Tome XVII. Nos. 4, 5. Tome XVIII. Nos. 1, 2. 4to. 1872.
*Scottish Society of Arts, Royal*—Transactions, Vol. VIII. Parts 3, 4. 8vo. 1871–3.
*Spilsbury, W. H. Esq. Librarian, Lincoln's Inn (the Author)*—Lincoln's Inn. 2nd edition. 16to. 1873.
*Squire, Balmanno, Esq. (the Author)*—Influence of Age on the Causation of Skin Disease. 16to. 1873.
*Stanford, Mr. E.*—M. Wagner: The Darwinian Theory, and the Laws of the Migration of Organisms, translated by J. Laird. (K 99) 8vo. 1873.
*Swanwick, Miss Anna, M.R.I. (the Translator)*—The Dramas of Æschylus. 2 vols. 16to. 1873.
*Symons, G. J. Esq. (the Author)*—Symons' Monthly Meteorological Magazine, June, 1873. 8vo.
*Telegraph Engineers, Society of*—Journal, Vol. I. No. 3. 8vo. 1873.
*United Service Institution, Royal*—Journal: Appendix to Vol. XVI. 8vo. 1873.
*Victoria Institute*—Annual Address of the President. 8vo. 1873.
*Yorkshire Archæological and Topographical Association*—Journal, Part 8. 8vo. 1873.
*Zoological Society of London*—Transactions, Vol. VIII. Parts 4, 5. 4to. 1873.
Proceedings, 1872, Part 3. 8vo.

## GENERAL MONTHLY MEETING,

Monday, Nov. 3, 1873.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The Secretary announced the decease of Sir Henry Holland, Bart. the President, on Oct. 27th.

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Henry Adolphus Focking, Esq.
Major John Andover Wood,

were *elected* Members of the Royal Institution.

The Special Thanks of the Members were given to Charles Woodward, Esq. F.R.S. for his Present of his work on the "Polarization of Light," and of much valuable apparatus illustrating the subject; and also to William Salmon, Esq. M.R.I. for his second Donation of Ten Pounds for the Promotion of Scientific Research in the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

From

*The India Office*—F. Day: Report on the Fresh-water Fish and Fisheries of India and Burma. 8vo. 1873.
*The French Government*—Documens Inédits sur l'Histoire de France: 1e Série, Histoire Politique: Négotiations de la France avec Toscane. Tome IV. 4to. 1872.—Lettres Missives de Henri IV. Tome VIII. 4to. 1872.
3e Série: Archéologie—Inscriptions de la France du Ve au XVIIIe Siècle. Tome I. 4to. 1873.
Monographie de la Cathédrale de Chartres: Atlas, 8e et 9e Livraisons. Fol. 1865.
Statistique Monumentale de Paris: Cartes, &c. 34e, 35e, et 36e Livraisons. Fol.
*The Spanish Government*—Estadistica Minera de España, 1870. 4to. 1873.
*Actuaries, Institute of*—Journal, No. 92. 8vo. 1873.
*American Association for the Advancement of Science*—Proceedings at the 20th Meeting, Aug. 1871. 8vo. 1872.
*Antiquaries, Society of*—Proceedings, Vol. VI. No. 1. 8vo. 1873.
*Asiatic Society of Bengal*—Journal, 1873, Part 1, No. 1; Part 2, Nos. 1, 2. 8vo. Proceedings, 1873, Nos. 1, 2, 3, 4. 8vo.
*Astronomical Society, Royal*—Monthly Notices, Vol. XXXIII. No. 8. 8vo. 1873.
*Basel Naturforschende Gesellschaft*—Verhandlungen, Vter Theil, 4tes Heft. 8vo. 1873.
*British Architects, Royal Institute of*—Sessional Papers, 1872–73. Nos. 13, 14. 4to.
*British Association for the Advancement of Science*—Report of the 42nd Meeting at Brighton, Aug. 1872. 8vo. 1873.
*British Museum Trustees*—Catalogue of Greek Coins—Italy. 8vo. 1873.
*Chemical Society*—Journal for July to Oct. 1873. 8vo.
*Clinical Society*—Transactions, Vol. VI. 8vo. 1873.

*Devonshire Association for the Advancement of Science*—Report and Transactions for 1873. Vol. VI. Part 1. 8vo. 1873.
*Editors*—American Journal of Science for July to Oct. 1873. 8vo.
 Athenæum for July to Oct. 1873. 4to.
 Chemical News for July to Oct. 1873. 4to.
 Engineer for July to Oct. 1873.
 Food Journal for July to Oct. 1873. 4to.
 Journal for Applied Science for July to Oct. 1873. Folio.
 Journal of Gas-Lighting for July to Oct. 1873. 4to.
 Nature for July to Oct. 1873. 4to.
 Nautical Magazine for July to Oct. 1873. 8vo.
 Once a Week for July to Oct. 1873. 8vo.
 Pharmaceutical Journal for July to Oct. 1873. 8vo.
 Telegraph Journal for July to Oct. 1873.
*Franklin Institute*—Journal, Nos. 570–573. 8vo. 1873.
*Geneva, Société de Physique*—Mémoires, Tome XXII. 4to. 1873.
*Geographical Society, Royal*—Proceedings, 1873, Nos. 3, 4, 5. 8vo. 1873.
 Journal, Vol. XLII. 8vo. 1872.
 The Lands of Cazembe. Lacerda's Journey to Cazembe in 1798, translated by R. F. Burton; &c. 8vo. 1873.
*Geological Institute, Imperial, Vienna*—Jahrbuch, 1873. Nos. 1, 2. 8vo.
 Verhandlungen, 1873. Nos. 1–9.
 Abhandlungen, Band V. Heft 4, 5. fol. 1873.
*Geological Society of Ireland*—Journal, N.S. Vol. III. Part 3. 8vo. 1873.
*Geological Society*—Quarterly Journal, No. 115. 8vo. 1873.
*Gore, George, Esq. F.R.S. (the Author)*—National Importance of Scientific Research. (West. Rev. April, 1873.)
*Henslow, Rev. George, M.A. (the Author)*—The Theory of the Evolution of Living Things and the Application of the Principle of Evolution to Religion. (An Actonian Prize Essay.) 12mo. 1873.
*Hills, Thomas Hyde, Esq. M.R.I.*—Dr. J. A. Hessey's Sermon on the Funeral of Sir E. Landseer. (K 99) 8vo. 1873.
*Hull Royal Institution*—Annual Report, 1873. 8vo.
*Iron and Steel Institute*—Journal, 1873, Vol. I. 8vo.
*Jablonowski'schen Gesellschaft*—Preisschrift XVII.—H. Zeissberg, Die Polnische Geschichtschreibung des Mittelalters. 8vo. 1873.
*Kirkaldy, David, Esq. (the Author)*—Results of an Experimental Inquiry into the Mechanical Properties of Fagersta Steel. 4to. 1873.
*Linnean Society*—Journal, Nos. 56, 73. 8vo. 1873.
 Proceedings, 1872–3. 8vo.
 Transactions, Vol. XXVIII. Part 3; Vol. XXIX. Part 2. 4to. 1873.
*Lowne, B. Thompson, M.R.C.S. F.L.S. (the Author)*—The Philosophy of Evolution. (An Actonian Prize Essay.) 16to. 1873.
*Lubbock, Sir John (the Author)*—Note on the Macas Indians. (Ethnol. Journal, II.) 8vo. 1873.
*Manchester Philosophical Society*—Proceedings, Vol. XII. Nos. 12, 13. Vol. XIII. Nos. 1, 2. 8vo. 1873.
*Marshall, Lieut.-Col. W. E. (the Author)*—A Phrenologist among the Todas, South India. 8vo. 1873.
*Mechanical Engineers' Institution, Birmingham*—Proceedings, May, 1873. 8vo.
*Mensbrugghe, G. Van der (the Author)*—Sur la Tension Superficielle des Liquides, 2nd Mémoire. 4to. 1873.
*Meteorological Committee of the Royal Society*—Report for 1872. 8vo. 1873.
*Meteorological Office*—Quarterly Weather Report, 1872. Part 3. 4to.
 Meteorology of the Antarctic Regions. 4to. 1873.
*Meteorological Society*—Quarterly Journal, New Series, No. 7. 8vo. 1873.
*Photographic Society*—Journal, No. 243. 8vo. 1873.
*Preussische Akademie der Wissenschaften*—Monatsberichte, März, April, Mai. 1873. 8vo

*Radford, W. T. M.D. M.R.I.*—G. B. Rinuccini: Embassy in Ireland, 1645–49; translated by Annie Hutton. 8vo. 1873.

*Rigg, Rev. A., M.A. (the Author)*—Easy Introduction to Chemistry. 16to. 1873.

*Rome, Accademia dei Lincei*—Atti, Anno XXVI. Sess. 1. 4to. 1873.

*Royal Irish Academy*—Transactions, Vol. XXIV. Science, Parts 16, 17. Vol. XXV. Science Parts 1–4. 4to. 1870–3.

Proceedings, Vol. X. Second Series, Vol. I. Nos. 2–8. 8vo. 1867–73.

*Royal Society of London*—Philosophical Transactions for 1872, Part 2. 4to. 1873. Proceedings, Nos. 145, 146. 8vo. 1873.

*Sabine, General Sir Edward, K.C.B. F.R.S. M.R.I. (the Author)*—Contributions to Terrestrial Magnetism, No. 13. (Phil. Trans. R.S. 1873.)

*Smithsonian Institution*—Smithsonian Contributions to Knowledge, Vol. XVIII. 4to. 1873.

*Statistical Society*—Journal, Vol. XXXVI. Parts 2, 3. 8vo. 1873.

*St. Pétersbourg, Académie Impériale des Sciences*—Mémoires, 7$^e$ Série, Tome XIX. Nos. 3–7. 4to. 1873.

Bulletins, Tome XVII. Nos. 4, 5. Tome XVIII. Nos. 1, 2. 4to. 1872.

*Symons, G. J. Esq. (the Author)*—Symons' Monthly Meteorological Magazine, July to Oct. 1873. 8vo.

*Taylor, Alfred S. M.D. F.R.S. M.R.I. (the Author)*—The Principles and Practice of Medical Jurisprudence. 2nd Ed. 2 vols. 8vo. 1873.

*Tyndall, Professor, LL.D. D.C.L. F.R.S. (the Author)*—Six Lectures on Light, delivered in America in 1872–1873. 16to. 1873.

*United Service Institution, Royal*—Journal, Nos. 72, 73. 8vo. 1873.

*Vereins zur Beförderung des Gewerbfleisses in Preussen*—Verhandlungen, Jan.–April. 1873. 4to.—F. Kohl, Geschichte der Jacquard-Maschine nebst der Biographie Jacquards. 4to. Berlin. 1872.

*Victoria Institute*—Journal, No. 26. 8vo. 1873.

*Vincent, B. (the Editor)*—Haydn's Dictionary of Dates and Universal Information. 14th Edition. 8vo. 1873.

*Woodward, Charles, Esq. F.R.S. (the Author)*—Familiar Introduction to the Study of Polarized Light. 3rd Edition. 12mo. 1861. [With much Optical Apparatus.]

Papers on Polarized Light, &c. By Dr. H. B. Leeson and Dr. Pereira. 8vo.

## GENERAL MONTHLY MEETING,

### Monday, Dec. 1, 1873.

GEORGE BUSK, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The following Letter to the Family of the late President was unanimously adopted by the Members present:—

"The MEMBERS OF THE ROYAL INSTITUTION beg to be permitted to express to the Family of their late President, SIR HENRY HOLLAND, their deepest sympathy in the great loss they have recently sustained.

"It is a loss in which the Institution largely shares; and it has to deplore a President esteemed by all for his distinguished scientific and literary attainments, for the abilities and varied experience he brought to the performance of his duties; and beloved for the uniform kindness and consideration with which he discharged them.

"In the published record of his life SIR HENRY HOLLAND has acknowledged that his connection with the Royal Institution had been very valuable to him. It is now for the Members of the Institution to testify their profound sense of the exceeding advantages conferred by that association upon the Royal Institution.

"In it, as he himself has related, he stood by the cradle of some of the most wonderful discoveries of the age, and watched their progress to maturity and fame. He was one of the earliest to witness the production of the Alkaline Metals by Davy, and among the first to see the small luminous spark elicited from the magnet by Faraday. In later times he was foremost in establishing the Royal Institution Research Fund, and was unceasingly generous in his own large personal contributions to it.

"As President he was always at his post, ever prompt with assistance and counsel; a sound adviser and a courteous friend. The recollection of his presence in the Institution will not soon be forgotten, and will long remain to be cherished by its Members with affection and regard.

"The close of such a career must be noted with no ordinary feelings of interest and regret, and the Members of the Royal Institution desire to assure the Family of their lamented President how fully they enter into the universal sorrow for his death."

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The DUKE OF NORTHUMBERLAND, D.C.L. was unanimously elected President of the Royal Institution, in the room of the late SIR HENRY HOLLAND.

Mrs. Walter Fawcett,
Charles Craddock Underwood, Esq.

were *elected* Members of the Royal Institution.

The LECTURE ARRANGEMENTS for the ensuing season were announced:—

PROFESSOR TYNDALL, D.C.L. LL.D. F.R.S. Six Lectures on THE MOTION AND SENSATION OF SOUND, on Dec. 27, 30, 1873; Jan. 1, 3, 6, 8, 1874.

### Before Easter, 1874.

PROFESSOR RUTHERFORD, M.D. F.R.S.E. Five Lectures on Respiration, on Tuesdays, Jan. 13 to Feb. 10.

[PROFESSOR RUTHERFORD will give Six Lectures on the Nervous System after Easter.]

PROFESSOR TYNDALL, D.C.L. LL.D. F.R.S. Six Lectures on the Physical Properties of Gases and Liquids, on Tuesdays, Feb. 17 to March 24.

PROFESSOR P. M. DUNCAN, F.R.S. Six Lectures on Palæontology, with reference to Extinct Animals and the Physical Geography of their Time, on Thursdays, Jan. 15 to Feb. 19.

PROFESSOR W. C. WILLIAMSON, F.R.S. Five Lectures on Cryptogamic Vegetation, on Thursdays, Feb. 26 to March 26.

PROFESSOR G. CROOM ROBERTSON, University College, London. Four Lecture on Kant's Critical Philosophy, on Saturdays, Jan. 17 to Feb. 7.

R. BOSWORTH SMITH, Esq. M.A. Four Lectures on Mohammed and Mohammedanism, on Saturdays, Feb. 14 to March 7.

CHARLES THOMAS NEWTON, Esq. M.A. Keeper of Greek and Roman Antiquities, British Museum. Three Lectures on Ephesus, on Saturdays, March 14, 21, and 28.

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The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

*The Lords of the Admiralty*—Nautical Almanac for 1877. 8vo. 1873.

*Agricultural Society Royal*—Journal, 2nd Series, Vol. IX. Part 2. 8vo. 1873.

*Antiquaries, Society of*—Proceedings Vol. V. No. 8. 8vo. 1873.

*Astronomical Society, Royal*—Monthly Notices, Vol. XXXIII. No. 9. 8vo. 1873.

*Bavarian Academy of Sciences, Royal*—Sitzungsberichte. 1872; Heft 3. 1873; Heft 1. 8vo.

*Beni-Barde, Dr. (the Author)*—Traité Théorique et Pratique d'Hydrothérapie. 8vo. Paris, 1873.

*British Architects, Royal Institute of*—Sessional Papers, 1873–74, No. 1. 4to.

*British Museum Trustees*—Catalogue of Hemiptera Heteroptera, Part 8. 8vo. 1873.

*Chemical Society*—Journal for Nov. 1873. 8vo.

*Dawson, J. W. Esq. LL.D. F.R.S. (the Author)*—Report on the Fossil Plants of Canada. 8vo. 1873.

*Editors*—American Journal of Science for Nov. 1873. 8vo.
  Athenæum for Nov. 1873. 4to.
  Chemical News for Nov. 1873. 4to.
  Engineer for Nov. 1873.
  Food Journal for Nov. 1873. 4to.
  Journal for Applied Science for Nov. 1873. Folio.
  Journal of Gas-Lighting for Nov. 1873. 4to.
  Nature for Nov. 1873. 4to.
  Nautical Magazine for Nov. 1873. 8vo.
  Once a Week for Nov. 1873. 8vo.
  Pharmaceutical Journal for Nov. 1873. 8vo.
  Telegraph Journal for Nov. 1873. 8vo.

*Geneva, Société de Physique*—Mémoires, Tome XXIII. Partie 1. 4to. 1873.

*Geological Society*—Quarterly Journal, No. 116. 8vo. 1873.

*Leighton, John, Esq. F.S.A. M.R.I.*—The New Testament in Ancient Slave. 16to.

*Manchester Philosophical Society*—Proceedings, Vol. XIII. Nos. 3, 4. 8vo. 1873.

*Medical and Chirurgical Society, Royal*—Transactions, Vol. LVI. 8vo. 1873.

*Meteorological Committee of the Royal Society*—Notes on the Cyclones in the South Indian Ocean. 8vo. 1873.

*Meteorological Society*—Quarterly Journal, No. 8. 8vo. 1873.

*Mivart, St. George, Esq. F.R.S. M.R.I. (the Author)*—Man and Apes. 12mo. 1873.

*Olivera, Carlos, Esq.*—Boletin de la Esposicion Nacional en Cordoba (Argentine Republic). 7 vols. 8vo. 1869–73.

*Owen, Professor, F.R.S. (the Author)*—Anatomy of the King Crab (Limulus Polyphemus). 4to. 1873.

*Photographic Society*—Journal, No. 244. 8vo. 1873.

*Preussische Akademie der Wissenschaften*—Monatsberichte, Juni–Aug. 1873. 8vo.

*Scott, Robert H. Esq. F.R.S. (the Author)*—On the Connection between Colliery Explosions and Weather in 1871. (Met. Soc. Journal, 1873.)

*St. Bartholomew's Hospital*—Reports, Vol. IX. 8vo. 1873.

*Victoria Institute*—Journal, Nos. 8 and 27. 8vo. 1873.

*Walford, Weston S. Esq. F.S.A. M.R.I.*—Papworth's Ordinary of British Armorials. Parts 19, 20, 21. 8vo. 1873.

# Royal Institution of Great Britain.

## WEEKLY EVENING MEETING,

Friday, January 16, 1874.

William Spottiswoode, Esq. LL.D. Treas. R.S. Secretary and Vice-President, in the Chair.

Professor Tyndall, D.C.L. LL.D. F.R.S.

### *On the Acoustic Transparency and Opacity of the Atmosphere.*

This discourse embraced the scientific results of an inquiry on Fog-signals, undertaken at the instance of the Elder Brethren of the Trinity House, and communicated with their friendly concurrence to the Royal Society and the Royal Institution.

The investigation was begun on the 19th of May, 1873, and continued till the 4th of July. It was resumed on the 8th of October, and continued to the end of November. It also includes observations made during the dense fog which enveloped London on December 9 and the succeeding days.

Gongs and bells were excluded from this investigation, in consequence of their proved inferiority to other instruments of signalling. The experiments were made with trumpets blown by powerfully compressed air, with steam-whistles, guns, and a steam-syren, associated with a trumpet 16 feet long.

Daboll's horn, or trumpet, had been highly spoken of by writers on fog-signals. A third-order apparatus of the kind had been reported as sending its sound to a distance of from 7 to 9 miles against the wind, and to a distance of 12 to 14 miles with the wind. Holmes had improved upon Daboll; and with two instruments of Holmes, not of the third but of the first order, our experiments were made. On the 19th of May, at 3 miles distance, they became useless as a fog-signal; at a distance of 4 miles, with paddles stopped and all on board quiet, they were wholly unheard. At a distance of 2 miles from the Foreland the whistles tested on May 19 became useless. The 12 o'clock gun, fired with a 1-lb. charge at Drop Fort in Dover, was well heard on May 19, when the horns and whistles were inaudible. On the 20th of May the permeability of the atmosphere had somewhat increased, but the steam-whistle failed to pierce it to a depth of 3 miles. At 4 miles the horns, though aided by quietness on board, were barely heard. By careful nursing, if I may use the expression, the horn-sounds were carried to a distance of 6 miles. The superiority of the

18-pounder gun, already employed by the Trinity House as a fog-signal, over horns and whistles was on this day so decided as almost to warrant its recommendation to the exclusion of all the other signals.

Nothing occurred on the 2nd of June to exalt our hopes of the trumpets and whistles. The horns were scarcely heard at a distance of 3 miles; sometimes, indeed, they failed to be heard at 2 miles. By keeping everything quiet on board, they were afterwards heard to a distance of 6 miles—a result, however, mainly due to the improved condition of the atmosphere. Considering the demands as to sound-range made by writers on this subject, the demonstrated incompetence of horns and whistles of great reputed power to meet these demands was not encouraging. On the 3rd of June the atmosphere had changed surprisingly. It was loaded overhead with dark and threatening clouds; the sounds, nevertheless, were well heard beyond 9 miles. On June 10 the acoustic transparency of the air was also very fair, the distance penetrated being upwards of 8¾ miles. The subsidence of the sound near the boundary of the acoustic shadow on the Dover side of the Foreland, and before entering the shadow, was to-day sudden and extraordinary, affecting equally both horns and guns. We were warned on June 3 that the supremacy of the gun on one occasion by no means implied its supremacy on all occasions; the self-same guns which on May 20 had so far transcended the horns being on this day their equals and nothing more. The 11th of June was employed in mastering still further the facts relating to the subsidence of the sound east and west of the Foreland; the cause of this subsidence being in part due to the weakening of the sonorous waves by their divergence into the sound shadow, and in part to interference.

The atmosphere on the 25th of June was again very defective acoustically. The sounds reached a maximum distance of 6½ miles; but at 4 miles, on returning from the maximum distance, they were very faint. The day had, as it were, darkened acoustically. On this day the guns lost still further their pre-eminence, and at 5½ miles were inferior to the horn. No sounds whatever reached Dover Pier on the 11th; and it was only towards the close of the day that they succeeded in reaching it on the 25th. Thus by slow degrees the caprices of the atmosphere made themselves known to us, showing us subsequently that within the limits of a single day, even within a single minute, the air, as a vehicle of sound, underwent most serious variations. The 26th of June was a far better day than its predecessor, the acoustic range being over 9¼ miles. The direction of the wind was less favourable to the sound on this day than on the preceding one, plainly proving that something else than the wind must play an important part in shortening the sound-range.

On the 1st of July we experimented upon a rotating horn, and heard its direct or axial blast, which proved to be the strongest, at a distance of 10½ miles. The sounds were also heard at the Varne light-ship, which is 12¾ miles from the Foreland. The atmosphere had become decidedly clearer acoustically, but not so optically; for on this day thick haze obscured the white cliffs of the Foreland. In fact, on days of far greater optical purity, the sound had failed to reach one-third of the distance attained to-day. In the light of such a fact, any attempt to make optical transparency a measure of acoustic transparency must be seen to be delusive. On the 1st of July a 12-inch American whistle, of which we had heard a highly favourable account, was tried in place of the 12-inch English whistle; but, like its predecessor, the performance of the new instrument fell behind that of the horns. An interval of twelve hours sufficed to convert the acoustically clear atmosphere of July 1 into an opaque one; for on the 2nd of July even the horn-sounds, with paddles stopped and all noiseless on board, could not penetrate farther than 4 miles.

Thus each succeeding day provided us with a virtually new atmosphere, clearly showing that conclusions founded upon one day's observations might utterly break down in the presence of the phenomena of another day. This was most impressively demonstrated on the day now to be referred to. The acoustic imperviousness of the 3rd of July was found to be still greater than that of the 2nd, while the optical purity of the day was sensibly perfect. The cliffs of the Foreland could be seen to-day at ten times the distance at which they ceased to be visible on the 1st, while the sounds were cut off at one-sixth of the distance. At 2 P.M. neither guns nor trumpets were able to pierce the transparent air to a depth of 3, hardly to a depth of 2 miles. This extraordinary opacity was proved conclusively to arise from the irregular admixture with the air of the aqueous vapour raised by a powerful sun. This vapour, though perfectly invisible, produced *an acoustic cloud* impervious to the sound, and from which the sound-waves were thrown back as the waves of light are from an ordinary cloud. The waves thus refused transmission produced by their reflexion echoes of extraordinary strength and duration. This I may remark is the first time that audible echoes have been proved to be reflected from an optically transparent atmosphere. By the lowering of the sun the production of the vapour was checked, and the transmissive power of the atmosphere restored to such an extent, that, at a distance of 2 miles from the Foreland, at 7 P.M., the intensity of the sound was at least thirty-six times its intensity at 2 P.M.

That layers of dried air alternating with layers of air saturated with the vapour of a volatile liquid can powerfully intercept sound, has been experimentally established.

On October 8 the observations were resumed, a steam-syren and a Canadian whistle of great power being added to the list of instruments. A boiler had its steam raised to a pressure of 70 lbs. to the square inch; on opening a valve this steam would issue forcibly in a continuous stream, and the sole function of the syren was to convert this stream into a series of separate strong puffs. This was done by causing a disk with 12 radial slits to rotate behind a fixed disk with the same number of slits.

When the slits coincided a puff escaped; when they did not coincide the outflow of steam was interrupted. Each puff of steam at this high pressure generated a sonorous wave of great intensity; the successive waves linking themselves together to a musical sound so intense as to be best described as a continuous explosion.

During the earlier part of October 8 the optical transparency of the air was very great; its acoustic transparency, on the other hand, was very defective. Clouds blackened and broke into a rain- and hail-shower of tropical violence. The sounds, instead of being deadened, were improved by this furious squall; and, after it had lightened, thus lessening the local noises, the sounds were heard at a distance of 7½ miles distinctly louder than they had been heard through the preceding rainless atmosphere at a distance of 5 miles. At 5 miles distance, therefore, the intensity of the sound had been at least doubled by the rain—a result entirely opposed to all previous assertions, but an obvious consequence of the removal by condensation and precipitation of that vapour the mixture of which with the air had been proved so prejudicial to the sound. On this day a dependence was established between the pitch of a note and its penetrative power—the syren generating 480 waves being slightly inferior to the horns, while generating 400 waves a second it was distinctly superior. The maximum range on October 8 was 9 miles. On October 9 the transmissive power had diminished, the maximum range being 7½ miles. On both these days the syren proved to be superior to the horns, and on some occasions superior to the gun.

On the 10th and 11th, a gale having caused our steamer to seek safety in the Downs, we made land observations. The duration of the aërial echoes was for the syren and the gun 9 seconds, for the horns 6 seconds. The duration varies from day to day. We sought to estimate the influence of the violent wind, and found that the sound of the gun failed to reach us in two cases at a distance of 550 yards to windward, the sound of the syren at the same time rising to a piercing intensity. To leeward the gun was heard at five times, and certainly might have been heard at fifteen times the distance attained to windward. The momentary character of the gun-sound renders it liable to be quenched by a single puff of wind; but sounds of low pitch generally, whether momentary or not, suffer more from an opposing wind than high ones. We had on the 13th another example of the powerlessness of heavy rain to deaden sound.

On the 14th the maximum range was 10 miles, but the atmosphere did not maintain this power of transmission. It was a day of extreme optical clearness; but its acoustic clearness diminished as the day advanced. In fact the sun was in action. We proved to-day that by lowering the pitch of the Canadian whistle, its sound, which had previously been inaudible, became suddenly audible. The day at first was favourable to the transmission of the longer sound-waves. After a lapse of three hours the case was reversed, the high-pitched syren being then heard when both guns and horns were inaudible.

But even this state of things did not continue, so rapid and surprising are the caprices of the atmosphere. At a distance of 5 miles, at 3.30 P.M., the change in transmissive power reduced the intensity of the sound to at least one-half of what it possessed at 11.30 A.M., the wind throughout maintaining the same strength and direction. Through all this complexity the knowledge obtained on July 3 sheds the light of a principle which reduces to order the apparent confusion.

October 15 was spent at Dungeness in examining the performance of Daboll's horn. It is a fine instrument, and its application was ably worked out by its inventor; still it would require very favourable atmospheric conditions to enable it to warn a steamer before she had come dangerously close to the shore. The direction in which the aërial echoes return was finely illustrated to-day, that direction being always the one in which the axis of the horn is pointed.

The 16th was a day of exceeding optical transparency, but of great acoustic opacity. The maximum range was only 5 miles. On this day the howitzer and all the whistles were clearly overmastered by the syren. It was, moreover, heard at 3½ miles with the paddles going, while the gun was unheard at 2½ miles. With no visible object that could possibly yield an echo in sight, the pure aërial echoes, coming from the more distant southern air, were distinct and long continued at a distance of 2 miles from the shore. Near the base of the Foreland cliff we determined their duration and found it to be 11 seconds, while that of the best whistle echoes was 6 seconds. On this day three whistles, sounded simultaneously, were pitted against the syren, and found clearly inferior to it.

On the 17th four horns were compared with the syren and found inferior to it. This was our day of greatest acoustic transparency, the sound reaching a maximum of 15 miles for the syren, and of more than 16 for the gun. The echoes on this day were continued longer than on any other occasion. They continued for 15 seconds, their duration indicating the atmospheric *depth* from which they came.

On October 18, though the experiments were not directed to determine the transmissive power of the air, we were not without proof that it continued to be high. From 10 to 10.30 A.M., while waiting for the blasts of the syren at a distance of 3 miles from the Foreland, the continued reports of what we supposed to be the musketry of skirmishing parties on land were distinctly heard by us all. We afterwards learned that the sounds arose from the rifle-practice on Kingsdown beach, 5½ miles away. On July 3, which, optically considered, was a far more perfect day, the 18-pounder, howitzer, and mortar failed to make themselves heard at half this distance. The 18th was mainly occupied in determining the influence of pitch and pressure on the syren-sound. Taking the fluctuations of the atmosphere into account, I am of opinion that the syren, performing from 2000 to 2400 revolutions a second, or, in other words, generating from 400 to 480 waves per second, best meets the atmospheric conditions. We varied the pressure from 40 to 80 lbs. on the square inch; and though the intensity did not appear to rise in proportion to the pressure, the higher pressure yielded the hardest and most penetrating sound.

The 20th was a rainy day with strong wind. Up to a distance of 5½ miles the syren continued to be heard through the sea- and paddle-noises. In rough weather, indeed, when local noises interfere, the syren-sound far transcends all other sounds. On various occasions to-day it proved its mastery over both gun and horns. On the 21st the wind was strong and the sea high. The horn-sounds, with paddles going, were lost at 4 miles, while the syren continued serviceable up to 6½ miles. The gun to-day was completely overmastered. Its puffs were seen at the Foreland; but its sound was unheard when the syren was distinctly heard. Heavy rain failed to damp the power of the syren. The whistles were also tried to-day, but were found far inferior to the syren. On the 22nd it blew a gale, and the 'Galatea' quitted us. We made observations on land on the influence of the wind and of local noises. The shelter of the Coastguard Station at Cornhill enabled us to hear gun-sounds which were quite inaudible to an observer out of shelter; in the shelter also both horn and syren rose distinctly in power; but they were also heard outside when the gun was quite unheard. As usual the sound to leeward was far more powerful than those at equal distances to windward. The echoes from the cloudless air were to-day very fine. On the 23rd, in the absence of the steamer, the observations on the influence of the wind were continued. The quenching of the gun-sounds, in particular, to windward was well illustrated. All the sounds, however, gun included, were carried much farther to leeward than to windward. The effect of a violent thunderstorm and downpour of rain in exalting the sound was noticed by observers both to windward and to leeward of the Foreland. In the rear of the syren its range to-day was about a mile. At right angles to the axis, and to windward, it was about the same. To leeward it reached a distance of 7⅓ miles.

On the 24th, when observations were made afloat in the steam-tug 'Palmerston,' the syren exhibited a clear mastery over gun and horns. The maximum range was 7¾ miles. The wind had changed from W.S.W. to S.E., then to E. As a consequence of this, the syren was heard loudly in the streets of Dover. On the 27th the wind was E.N.E.; and the syren-sound penetrated everywhere through Dover, rising over the moaning of the wind and all other noises. It was heard at a distance of 6 miles from the Foreland on the road to Folkestone, and would probably have been heard all the way to Folkestone had not the experiments ceased. Afloat and in the axis, with a high wind and sea, the syren, and it only, reached to a distance of 6 miles; at 5 miles it was heard through the paddle-noises. On the 28th further experiments were made on the influence of pitch, the syren when generating 480 waves a second being found more effective than when generating 300 waves a second. The maximum range in the axis on this day was 7½ miles.

The 29th of October was a day of extraordinary optical transparency, but by no means transparent acoustically. The gun was the greatest sufferer. At first it was barely heard at 5 miles; but afterwards it was tried at 5½, 4½, and 2½ miles, and was heard at none of these distances. The syren at the same time was distinctly heard. The sun was shining strongly; and to its augmenting power the enfeeblement of the gun-sound was doubtless due. At 3½ miles, subsequently, dead to windward, the syren was faintly heard; the gun was unheard at 2¾ miles. On land the syren and horn-sounds were heard to windward at 2 to 2½ miles, to leeward at 7 miles; while in the rear of the instruments they were heard at a distance of 5 miles, or five times as far as they had been heard on October 23.

The 30th October furnished another illustration of the fallacy of the notion which considers optical and acoustic transparency to go hand in hand. The day was very hazy, the white cliffs of the Foreland at the greater distances being quite hidden; still the gun- and syren-sounds reached on the bearing of the Varne light-vessel to a distance of 11½ miles. The syren was heard through the paddle-noises at 9¼ miles, while at 8½ miles it became efficient as a signal with the paddles going. The horns were heard at 6¼ miles. This was during calm. Subsequently, with a wind from the N.N.W., no sounds were heard at 6½ miles. On land, the wind being across the direction of the sound, the syren was heard only to a distance of 3 miles N.E. of the Foreland; in the other direction it was heard plainly on Folkestone Pier, 8 miles distant. Both gun and horns failed to reach Folkestone.

Wind, rain, a rough sea, and great acoustic opacity characterized October 31. Both gun and horns were unheard 3 miles away, the syren at the same time being clearly heard. It afterwards forced its sound with great power through a violent rain-squall. Wishing the same individual judgment to be brought to bear upon the sounds on both sides of the Foreland, in the absence of our steamer, which had quitted us for safety, I committed the observations to Mr. Douglass. He heard them at 2 miles on the Dover side, and on the Sandwich side, with the same intensity, at 6 miles.

A gap (employed by the engineers in making arrangements for pointing the syren in any required direction) here occurred in our observations. They were resumed, however, on November 21, when comparative experiments were made upon the gun and syren. Both sources of sound, when employed as fog-signals, will not unfrequently have to cover an arc of 180°; and it was desirable to know with greater precision how the sound is affected by the direction in which the gun or syren is pointed.

The gun, therefore, was in the first instance pointed on us and fired, then turned and fired along a line perpendicular to that joining us and it. There was a sensible, though small, difference between the sounds which reached us in the two cases. A similar experiment was made with the syren; and here the falling off when the instrument was pointed perpendicular to the line joining us and it was very considerable. This is what is to be expected; for the trumpet associated with the syren is expressly intended to gather up the sound and project it in a certain direction, while no such object is in view in the construction of the gun. The experiments here referred to were amply corroborated by others made on November 22 and 23.

On both of these days the 'Galatea's' guns were fired to windward and to leeward. The aërial echoes in the latter case were distinctly louder and longer than in the former. The experiment has been repeated many times, and always with the same result.

In front of the Cornhill Coastguard Station, and only 1¼ mile from the Foreland, the syren, on the 21st, though pointed towards us, fell suddenly and considerably in power. Before reaching Dover Pier it had ceased to be heard. The wind was here against the sound; but this, though it contributed to the effect, could not account for it, nor could the proximity of the shadow account for it. To these two causes must have been added an acoustically flocculent though optically transparent atmosphere. The experiment demonstrates conclusively that there are atmospheric and local conditions which, when combined, prevent our most powerful instruments from making more than a distant approach to the performance which writers on fog-signals have demanded of them.

On November 24 the sound of the syren, pointed to windward, was compared at equal distances in front of and behind the instrument. It was louder to leeward in the rear than at equal distances to windward in front. Hence, in a wind, the desirability of pointing the instrument to windward. The whistles were tested this day in comparison with the syren deprived of its trumpet. The Canadian and the 8-inch whistles proved the most effective; but the naked syren was as well heard as either of them. As regards opacity, the 25th of November almost rivalled the 3rd of July. The gun failed to be heard at a distance of 2·8 miles, and it yielded only a faint crack at 2½ miles.

Meanwhile this investigation has given us a knowledge of the atmosphere in its relation to sound, of which no notion had been previously entertained. While the *velocity* of sound has been the subject of refined and repeated experiments, I am not aware that since the publication of a celebrated paper by Dr. Derham in the 'Philosophical Transactions' for 1708, any systematic inquiry has been made into the causes which affect the *intensity* of sound in the atmosphere. Derham's results, though obtained at a time when the means of investigation were very defective, have apparently been accepted with unquestioning trust by all subsequent writers—a fact which is, I think, in some part to be ascribed to the *a priori* probability of his conclusions.

Thus Dr. Robinson, relying apparently upon Derham, says, "Fog is a powerful damper of sound," and he gives us physical reason why it must be so. "It is a mixture of air and globules of water, and at each of the innumerable surfaces where these two touch, a portion of the vibration is reflected and lost." And he adds further on, "The remarkable power of fogs to deaden the report of guns has been often noticed."

Assuming it, moreover, as probable that the measure of "a fog's power in stopping sound" bears some simple relation to its opacity for light, Dr. Robinson, adopting a suggestion of Mr. Alexander Cunningham, states that "the distance at which a given object, say a flag or pole, disappears, may be taken as a measure of the fog's power" to obstruct the sound. This is quite in accordance with prevalent notions; and granting that the sound is dissipated, as assumed, by reflexion from the particles of fog, the conclusion follows that the greater the number of the reflecting particles, the greater will be the waste of sound. But the number of particles, or, in other words, the density of the fog, is declared by its action upon light; hence the optical opacity will be a measure of the acoustic opacity.

This, I say, expresses the opinion generally entertained, "clear, still air" being regarded as the best vehicle for sound. We have not, as stated above, experimented in really dense fogs; but the experiments actually made entirely destroy the notion that clear weather is necessarily better for the transmission of sound than thick weather. Some of our days of densest acoustic opacity have been marvellously clear optically, while some of our days of thick haze have shown themselves highly favourable to the transmission of sound. Were the physical cause of the sound-waste that above assigned, did that waste arise in any material degree from reflexion at the limiting surfaces of the particles of haze, this result would be inexplicable.

Again; Derham, as quoted by Sir John Herschel, says that "falling rain tends powerfully to obstruct sound." We have had repeated reversals of this conclusion. Some of our observations have been made on days when rain and hail descended with a perfectly tropical fury; and in no single case did the rain deaden the sound; in every case, indeed, it had precisely the opposite effect.

But falling snow, according to Derham, offers a more serious obstacle than any other meteorological agent to the transmission of sound. We have not extended our observations at the South Foreland into snowy weather; but an observation of my own made on December 29, in the Alps, during a heavy snowstorm, distinctly negatives the statement of Derham.

Reverting to the case of fog, I am unable in modern observations to discover anything conclusive as to its alleged power of deadening sound. I had the pleasure of listening to a very interesting lecture on fog-signals delivered by Mr. Beazeley before the United Service Institution; and I have carefully perused the printed report of that lecture, and of a paper previously communicated by Mr. Beazeley to the Institution of Civil Engineers. But in neither of these painstaking compilations can I find any adequate evidence of the alleged power of fogs to deaden sound.

Indeed during the discussion which followed the reading of Mr. Beazeley's paper, an important observation in an opposite sense was mentioned by Mr. Douglass, to whose ability and accuracy as an observer I am able to bear the strongest testimony. Mr. Douglass stated that he had found in his experience but little difference in the travelling of sound in foggy or in clear weather. He had distinctly heard in a fog, at the Smalls rock in the Bristol Channel, guns fired at Milford Haven, 25 miles away. Mr. Beazeley, moreover, has heard the Lundy-Island gun "at Hartland Point," a distance of 10 miles, during dense fog. Mr. Beazeley's conclusion, indeed, accurately expresses the state of our knowledge when he wrote. In winding up his paper, he admitted "that the subject appeared to be very little known, and that the more it was looked into the more apparent became the fact that the evidence as to the effect of fog upon sound is extremely conflicting." When, therefore, it is alleged, as it is so often alleged, that the power of fogs to deaden sound is well known, the disjunctive *not* is to be inserted before the predicate.

The real enemy to the transmission of sound through the atmosphere has, I think, been clearly revealed by the foregoing inquiry. That enemy has been proved to be not rain, nor hail, nor haze, nor fog, nor snow—not water in fact in either a liquid or a solid form, but water in a vaporous form, mingled with air so as to render it acoustically turbid and flocculent. This acoustic turbidity often occurs on days of surprising optical transparency. Any system of measures, therefore, founded on the assumption that the optic and acoustic transparency of the atmosphere go hand in hand must prove delusive.

There is but one solution of this difficulty: it is to make the source of sound so powerful as to be able to endure loss by partial reflexion, and still retain a sufficient residue for transmission. Of all the instruments hitherto examined by us the syren comes nearest to the fulfilment of this condition; and its establishment upon our coasts will, in my opinion, prove an incalculable boon to the mariner.

An account of the observations made during the recent fog adds the force of demonstration to others recorded in the paper, that fogs possess no such power of stifling sound as that hitherto ascribed to them. Indeed, the melting away of fog on December 13 was accompanied by an acoustic darkening of the atmosphere, so great that, at a point midway between the eastern end of the Serpentine, where a whistle was sounded, and the bridge, the sound possessed less than one-fourth of the intensity which it possessed on the day of densest fog.

Thus, I think, has been removed the last of a congeries of errors which for more than a century and a half have been associated with the transmission of sound by the atmosphere. [J. T.]

## WEEKLY EVENING MEETING,

Friday, January 23, 1874.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

J. J. Sylvester, Esq. M.A. LL.D. F.R.S.

CORRESPONDING MEMBER OF THE INSTITUTE OF FRANCE.

### *On recent Discoveries in Mechanical Conversion of Motion.*

The speaker stated that the subject he proposed to bring under the notice of the meeting related mainly to the discovery of a perfect parallel motion,—that is to say, of a mode of producing motion in a straight line by a system of pure link-work without the aid of grooves or wheel-work, or any other means of constraint than that due to fixed centres, and joints for attaching or connecting rigid bars. This important discovery was made by M. Peaucellier, an officer of Engineers in the French army [*]—and first published by him, in the form of a question, in the 'Annales de Mathématique' in the year 1864, and subsequently formed the subject of two communications to the "Société Philomathique" of Paris by Captain Manheim, but seems not to have received the attention it deserved from that learned body, and may be said to have passed into oblivion; so much so, that when rediscovered by a young student of the University of St. Petersburg, of the name of Lipkin, several years subsequently, the discovery was attributed to Lipkin instead of to Peaucellier even in works published in the French language, and so recently as 1873 by M. Colignan, in his 'Traité de Cinématique.' The eminent Professor Tchebicheff had long occupied himself with the question, but with less than his usual success in overcoming difficulties insuperable to the rest of the world. Lipkin was a student in his class, and may thus have had his attention turned to the question; at all events, Professor Tchebicheff's warm interest in the subject was displayed by his bringing Lipkin's name before the Russian Government, and securing for him a substantial reward for his supposed original discovery. Before Peaucellier's time all so-called parallel motions were imperfect, and gave merely approximate rectilinear motion;* in substance they will be without exception found to be merely modifications of Watt's original construction, and to depend on the motion of a point in, or rigidly connected with, a bar joining the extremities of two other bars rotating round fixed centres, which may be described briefly as three-bar motion. Peaucellier's exact parallel motion depends on a link-work of seven bars moving like Watt's, and the other imperfect parallel motions of the same class round two fixed centres.*

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[*] Now Colonel Peaucellier, and in command of the fortress of Toul; at the time of his discovery lieutenant and officier d'ordonnance on the staff of the "illustrious Marshal Niel."

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* The late lamented Professor Rankine, in his treatise on Millwork, and elsewhere, mentions a so-called "exact parallel motion," the invention of which he dubiously assigns to Mr. Scott Russell. In its *exact* form this is no parallel motion at all, for it works by means of a slide, and in its modified form it ceases to be *exact*, the motion produced being no longer truly rectilinear.

Mr. Kaulbach, a mechanical draughtsman, resident in London, has shown the speaker a sketch of a very ingenious *quasi*-parallel motion, which he took the first steps to patent a year or two ago, but has not thought it worth his while to proceed with it further. Its principle depends upon finding a curve made to rotate about a fixed point, and enjoying the property that the tangent to each point of it, as that point passes a given vertical line, shall take up a horizontal position. A piston-rod is guided in the direction of such vertical line, and the beam, which always presses on a friction wheel attached to the rod, is so shaped in its outward contour as to satisfy the above condition; the consequence is that the reaction on the piston-rod can only take effect vertically, *i. e.* in the direction of its motion, and no lateral pressure is produced.

Peaucellier's invention effects the perfect conversion of circular into linear motion. An easy practical deduction from this is the conversion of spherical into plane motion, by aid of universal joints and other familiar modes of effecting free motion in space, of a shaft about a fixed point or round another shaft. The announcement of these facts has occasioned many persons unacquainted with the technical language of mechanism to suppose that the discovery of Peaucellier is connected with the quadrature of the circle or cubature of the sphere, and led to the idea that the speaker was in possession of some secret for flattening spheres and turning circles into right lines. Such a misconception was one (as indeed the wide extent of its prevalence demonstrates) quite likely to occur even to intelligent persons untrained in mathematical science. Technical names are a frequent occasion of traps to the uninitiated. A lady present at one of Mr. Norman Lockyer's course of lectures on Spectral Analysis, near the close of it was overheard inquiring with some anxiety as to "when the spectres might be expected to make their appearance." Names are of course all important to the progress of thought, and the invention of a really good name, of which the want, not previously perceived, is recognized, when supplied, as having ought to be felt, is entitled to rank on a level in importance with the discovery of a new scientific theory. Imagine *plane*, *straight*, *circle*, and you are potentially a geometer. Think the meaning of the one word *Syzygy*, and the logic of algebra has become part of your being. But, on the other hand, there are cases where over-naming does harm. The speaker has no doubt that if reading music on the piano with the fingers were taught without the intervention of learning the names of the notes, twice the velocity of execution (and quick reading is here the *sine-quâ-non* for the existence of every other kind of excellence) might be acquired in half the time required under the present system. The names of the notes of course would have to be learned at a later stage as a medium for discourse; but they should not be used as a vehicle for obtaining command of digitation, as such use amounts to throwing upon the brain the labour of going through two steps when one would suffice, and the passage of a direct nervous current from the eye to the touch in the act of reading, even at an advanced stage, becomes by force of habit interrupted and diverted into a broken channel. The new method for learning to read on the pianoforte here suggested may be distinguished as the abnominal or undenominational or tactile method. The writer is prepared to show in detail how it can be carried out in practice.

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* The perfect parallel motion of Peaucellier looks so simple and moves so easily that people who see it at work almost universally express astonishment that it waited so long to be discovered. The idea of the facility of the result by a natural mental illusion gets transferred to the process of conception, as if a healthy babe were to be accepted as proof of an easy act of parturition. No impression can be more erroneous. The speaker, on the contrary, the more he reflects upon the problem that was to be solved, and the nature of the solution (essentially a process of transformation operating on polar co-ordinates), wonders the more that it was ever found out, and can see no reason why it should have been discovered for a hundred years to come. Viewed *a priori* there was nothing to lead up to it. It bears not the remotest analogy (except in the fact of a double centring) to Watt's parallel motion or any of its progeny. In the three-bar motion the two fixed points are so to say one as good as the other, there is no distinction to be drawn between them; whereas the two fixed centres (hereafter designated as the fulcrum and pivot) in Peaucellier's seven-bar arrangement are absolutely dissimilar in position and function. Peaucellier's apparatus naturally resolves itself into a cell and a spare link; no such decomposition presents itself in the three-bar motion. Again, looking at the matter *a posteriori*, it occurs to many well-grounded mathematicians to suppose that, as the most general motion of a link-work of seven or any number of bars for each possible mode of conjunction and centring must be capable of being expressed by a general algebraical equation, the particular combination for rectilinear motion, when such motion is possible, ought to be contained therein and inferrible therefrom by studying under what conditions the characteristic of the general equation can degenerate into a power of a linear function or, as might perhaps happen (and would be sufficient if it did), into such power multiplied by a function incapable of changing its sign. But the answer to this is that *practically* there could be little or no hope of ever obtaining the general equation. In one-bar motion the general curve (*i.e.* a circle) is of the 2nd order; in three-bar motion, as is well known, of the 6th order; very likely, therefore, in five-bar motion it would be of the 24th order at least; and in seven-bar motion, of the 120th order at least. The equation or system of equations of the 120th order, supposed to be applicable to seven-bar motion, one could hardly dream of obtaining, or of being able to manipulate if obtained. Written out at full length in a handwriting of moderate size, the area of a very large room might be insufficient to contain the whole of its terms, which would consist of 7381 groups, and might be tens or hundreds of thousands in number. No; it must either have been fallen upon in a chance or experimental way, and subsequently verified theoretically, or else hit off in some sudden glow of insight akin to but of a much intenser degree of illumination than that under which Professor Stokes was able to see that the hydrodynamical theorem of Lagrange before him, proved imperfectly by its author and others, and correctly but with great difficulty by Cauchy, was an immediate inference from the pretty nearly self-obvious fact of the complete time-derivatives of the three quantities to be proved *if ever then always zero*, being by virtue of the well-known general hydrodynamical equations, syzygetic functions of these quantities themselves. Dr. Tchebicheff has informed the writer that he has succeeded in proving the non-existence of a five-bar link-work capable of producing a perfect parallel motion; he is probably therefore in possession of the actual numerical order of the general equation or system of equations applicable to this case. It is not proved, and may not be true, that Peaucellier's is the only seven-bar link-work that will solve the problem of a perfect parallel motion. Who shall say whether there may not exist some other combination of seven bars in which the same or an analogous zig-zag symmetry to that which exists in the three-bar arrangement

To understand the principle of Peaucellier's link-work, it is convenient to consider previously certain properties of a linkage* (to coin a new and useful word of general application), consisting of an arrangement of six links, obtained in the following manner:—first conceive a rhomb or diamond formed by four equal links joined to one another; and now suppose a pair of equal links to be joined on to two opposite angles of such figure and to each other. All six links are supposed to lie (and to be constrained by the nature of their attachments to remain) in the same plane. The point of junction of the last-named pair of links (which it will be found convenient to call the fulcrum), according as they are greater or smaller than the sides of the diamond, will lie outside or inside the diamond. The linkage consisting of the six links may be termed a positive *cell* in the one case and a negative *cell* in the other.† It is easily seen, as a geometrical necessity, that the fulcrum,

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may reappear! This is a point which should not be allowed to remain subject to doubt.

* A link-work consists of an odd number of bars, a linkage of an even number. A linkage may be converted into a link-work *additively* by fixing one point of it as a fulcrum and attaching a second point disconnected from the first by a new link to another fulcrum, or *ablatively* by fixing two ends of a link, which may then be removed. When one point only of a linkage is fixed, any other point may be made to describe an arbitrary curve, but then the path of every other point becomes prescribed. In order for a combination of links to fulfil this so to say fatalistic condition, and to entitle it to the name of a linkage in the speaker's sense, which when greater precision is required may be distinguished as a *perfect* linkage, equivalent to the French *système de tiges à liaison complète*, a numerical relation must be satisfied between the number of links and the number of joints, *viz.* three times the number of links must be four greater than twice the number of joints. In applying this rule it must be understood that, if three links are jointed together, the junction counts for two joints; if four are jointed together, for three joints; and so on. A compass or a pair of scissors is the simplest kind of linkage; a set of lazy-tongs is another; a Peaucellier cell, subsequently described in the text, a third. If no three joints lie on the same link, the above numerical relation between joints and links may be stated in another form, *viz.* twice the number of joints is four greater than the number of links. But in applying the rule in this form all joints count alike as units, and for a simple compass the ends must be reckoned as joints.

† Mr. Penrose, the eminent architect and surveyor to St. Paul's Cathedral, the scientific expositor and elucidator in succession to Mr. Pennethorne of the surprising law of curvilinearity in the temples of the Greeks, has put up a house-pump worked by a negative Peaucellier-cell, to the great wonderment of the plumber employed, who could hardly believe his senses when he saw the sling attached to the piston-rod moving in a true vertical line, instead of wobbling as usual from side to side. There seems to be no reason why the perfect parallel motion should not be employed with equal advantage in the construction of ordinary water-closets. The author has been admitted to see the geometrical pump at work in Mr. Penrose's kitchen at Wimbledon. A sister pump of the ordinary construction stands beside it. The former, although quite as compact as its neighbour, throws up a considerably larger head of water with the same sweep of the handle. Its elegance, and the frictionless ease with which it can be worked (beauty as usual the stamp and seal of perfection) have made it the pet of the household. Some circular steps outside St. Paul's Cathedral very lately requiring repair, Mr. Penrose employed a circulo-circularly-adjusted Peaucellier cell to cut out templets in zinc for the purpose. The radius of the steps is about

in whatever way the linkage is moved about, will always lie in a straight line with the two free angles of the diamond, which may be called its poles, and the distances of these poles from the fulcrum, or

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40 feet, but to the great comfort and delectation of his clerk of the works, they were able to operate with a radius of not more than 6 or 7 feet in length. General Sir H. James, R.E., lately gave a lecture on the subject at Southampton, and informs the writer that this has been the means of inducing a gentleman of fortune residing there, well known in the yachting world, to fit up a marine engine with a Peaucellier parallel motion to use on board a steam yacht.

A very good idea of the form and operation of a negative cell may be gained by putting together the fore-fingers and ring-fingers of the two hands, and placing one middle finger a little over the other so as to keep all six fingers in the same plane. The first Peaucellier cell constructed in this country was a positive one, made by the speaker's friend, the eminent musician and inventor of the laryngoscope, Mr. Manuel Garcia, Ph.D., who happened to visit him shortly after his memorable interview with Dr. Tchebicheff, in which that great mathematician announced in answer to his inquiries after the progress of the disproof of the impossibility of the exact conversion of circular into rectilinear motion, which had so long occupied the attention of his illustrious guest, that it, the thing itself, not the proof of its impossibility, had been actually effected in France, and subsequently in Russia, by a freshman student in his own class. He showed Mr. Garcia the drawing of the cell and mounting left by Tchebicheff, and the next day was gratified by receiving from him a model constructed with a few pieces of wood, fastened together with nails as pivots, which, rough as it was, worked perfectly, and drew forth the most lively expressions of admiration from some of the most distinguished members of the Philosophical Club of the Royal Society (not mathematicians, but naturalists, geologists, chemists, and physicists), when it was brought in with the dessert, to be seen by them after dinner, as is the laudable custom among the members of that eminent body in making known to each other the latest scientific novelties. Presently after the speaker exhibited the same model in the hall of the Athenæum Club to his brilliant friend Sir William Thomson, of Glasgow, who nursed it as if it had been his own child, and when a motion was made to relieve him of it, replied, "No! I have not had nearly enough of it—it is the most beautiful thing I have ever seen in my life." This rude but invaluable model ought to be preserved in some physical laboratory as a historical relic. It served as an instrument by which the speaker in every case where it was seen gained immediate converts to the belief of the importance of Peaucellier's great discovery, whereas a mere geometrical diagram would have been as little regarded as a figure of the celebrated asses' bridge in Euclid at last, so great is the difference of the impression produced on the practical English mind by the *esse* and the *posse*—being told how a thing ought to act, and seeing it actually going. Considering the extraordinary conversions worked with Mr. Garcia's model, it would not be unsuitable to write in letters of gold on the board attached to it which gives support to the two frail centres, the famous motto of Constantine—"In hoc signo vinces."

*Apropos* of the mistaken impressions of great men. Did not Newton live and die in the belief of the incurability of chromatic dispersion; Cayley, affirm the infinitude of the number of the aszygetic invariants of binary quantics beyond the sixth order, thereby arresting for many years the progress of the triumphal car which he had played a principal part in setting in motion; Pontecoulant the possibility of the existence of a rotating fluid ellipsoid of equilibrium for other than forms of revolution?

And as regards the speaker himself, twenty years ago he emitted in the 'Philosophical Magazine' a conjectural criterion for distinguishing *a priori*, geometrical propositions capable only of indirect demonstration from those susceptible of direct, when, lo and behold! but a few days ago came over a seemingly incontrovertible refutation of his supposed law, addressed to the Vice Chancellor the ideal lines which represent those distances, may be called the arms of the cell. It is upon the geometrical relation between these arms that the remarkable mechanical properties of Peaucellier's cell depend. The cell may be made to change its form like a set of lazy-tongs or any other kind of linkage, by closing or opening the diamond: as this is done evidently the lengths of the arms alter; but it will be found, and is capable of easy geometrical proof, that they remain subject to a very simple condition, *viz.* one increases just as much as the other decreases, so that their product remains invariable; this product is equal to the difference between the square of either of the links (called the connectors) proceeding to the fulcrum and the square of any side of the diamond, to which we may give the name of the modulus of the cell. The speaker illustrated this property experimentally, using a negative cell for the purpose. When the fulcrum was midway between the two poles each arm was 12 inches in length. When one arm was made 18 inches the other was found to be 8; when again it was stretched to the length of 24 inches the other was 6, and so on, the product of the two remaining always 144; or, reckoning in feet, to the lengths 1, $\frac{3}{2}$, 2, 3 of one arm corresponded the lengths 1, $\frac{2}{3}$, $\frac{1}{2}$, $\frac{1}{3}$ of the other; showing that the length of one arm was so governed by the length of the other as that the numbers denoting the two were always inverse or reciprocal to each other when the modulus is taken as unity. Hence a Peaucellier's cell may be conveniently termed a Reciprocator or Inverter. If we were to suppose the connectors at their free ends, instead of being attached to the side angles of the diamond, to be joined on to two adjoining sides in such a manner as to become parallel to the other pair of sides, this parallelism would continue to subsist for all positions of the linkage, and the arms or distances of the fulcrum from the opposite angles or poles of the diamond would still remain in the same right line, but the relation between them would now be one of direct instead of inverse proportion. Conceive the fulcrum in such an arrangement to become fixed. Since we can not only alter the angles of the diamond, but make the whole arrangement turn round the fixed point, we can make either pole describe any plane curve whatever: the other pole will then describe a curve precisely similar in shape, but drawn on a different scale, as in any ordinary pantigraph.*

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of our University of Cambridge, (as a sort of Patriarch of the West, and recognized Official Defender of the Faith (as it is in Euclid) for the British isles), by Miss Chart, of Oakland, California, U.S., which it is to be hoped will speedily appear in the same journal where the erroneous hypothetical dogma first saw the light. His sin, after so long a delay, and travelling half round the world in the interim, has found him out. It ought to be added that Miss Chart does not claim for herself the merit of the refutation, but represents herself as having received it some years ago from a gentleman bearing the, to geometrical ears, auspicious-sounding name of Hesse.

* Sometime, according to the authority of a questionist in the 'Educational Times' for the current month, called a Pentegram. Theoretically two Peaucellier cells are equivalent to a Pentegram (for we may change $r$ into $\frac{K}{r}$ by one, and

But if we revert to the Peaucellier cell or Reciprocator, whether of the positive or negative form, and treat it in the same manner as the supposed pantigraphic arrangement, fixing the fulcrum, and making one of the poles—*i. e.* an extremity of one of the arms—describe any plane curve, the other pole will no longer describe a similar curve, but what in the language of geometry is termed an inverse of the curve in question, the fulcrum being the origin of the inversion.

Suppose now one of the poles is made to describe a circle, the other will describe the inverse of a circle, which geometricians are well aware will in general be another circle, subject to the exception that if the arc described by one pole is part of a circle passing through the fulcrum, which is here the origin of the inversion, the path of the second pole will be no longer a circle, but a perfect straight line, which, under a mathematical point of view, may be regarded as a circle with an infinite radius. If then, in addition to fixing the fulcrum, we still further constrain the motion of the Peaucellier cell by attaching one of the poles to a centre (which for the sake of distinction from the other fixed point above defined we may term the *pivot*) round which it can revolve, situated at an equal distance from that pole and the fulcrum, the other pole will describe a perfect straight line perpendicular to the line joining the fulcrum and the pivot. We have thus a combination of seven radiating bars attached to two fixed centres, one point of which describes a true rectilinear path, and thus the long-sought-for problem of a perfect parallel motion meets for the first time its complete solution.*

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that into K $r$ by the other), but whilst combinations of the former are adequate to the transformation of $r$ into any algebraic function of $r$, the latter are absolutely sterile, leading only to the one single sort of transformation (if it may be called so), $r$ into K $r$. It seems then going too far to say (as does the writer alluded to above) that the germ of Peaucellier's invention is contained in the Pentegram.

* The centre above spoken of may be taken in the line itself, which joins the poles and the fulcrum. If it be taken not too far out of this position of symmetry it will in the course of the motion be brought into such position; but if it be taken at starting (as it may be), at a sufficiently great distance from the cell, the position of symmetry may never be attained throughout the whole possible course of the motion. This circumstance has been generally overlooked, and accordingly too narrow a rule has been given for the construction of a Peaucellier parallel motion, *viz.* it is laid down that the pivot is to be taken midway between the fulcrum and one of the poles for some certain position of the instrument. The position of the fulcrum relative to the two poles gives rise to the distinction between a negative and positive cell; but the preceding remark shows that there is a further subdivision of Peaucellier parallel motions depending on the length of the mounting radius, and that positive and negative mounted cells each of them embrace two radically different forms or genera, which may be distinguished as the symmetrical and non-symmetrical respectively; in the one form there exists a position where the *first* lies in the line containing the *fulcrum* and the two poles, in the other, no such position can be found. In the ordinary rule given for the construction of a P. P. M. only the former of these two genera is included which, as machines, differ between themselves as much as do the ellipse and hyberbola as curves.

The speaker illustrated these results by various models constructed in wood. By changing the length of the radial bar connecting one pole of the cell with a fixed point, the free pole was shown to describe arcs of circles convex or concave to the fulcrum,

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It ought to be added that the motion of the *parallel-point* is always perpendicular to the line of centres, and in every position makes, with the line containing the fulcrum and the poles an angle equal to the angle contained in the segment of the circle (of which one pole describes an arc), which lies between it and the fulcrum. If we join the two fixed centres by a new link, and then unfix them, we obtain a linkage of eight bars, possessed of very remarkable properties, of one of which Peaucellier has availed himself to obtain a mechanical description of the Limaçon of Pascal, which is the inverse of a conic in respect to the focus as the organ of inversion.

By a combination of such linkages it is possible to cause any number of points, otherwise free, to remain always in a straight line with each other. The speaker believes that he is in possession of a *bonâ fide* valid proof of the proposition assumed on totally insufficient grounds by Peaucellier, *viz.* that every algebraical curve may be best described by link-work. The proof is founded on the union of the above statement (or still better, one founded on his own Kinomatical Paradox subsequently referred to) with Grassman's method of describing algebraical curves by means of an apparatus of fixed points and lines; this proposition, as far as concerns curves of the first nine genera (*i. e.* of a *cursality*, or, so to say, circuit-*complexity* not transcending the 9th degree), and also for curves of the first six orders, or for any order where the degree of one of the variables in the representing equation is 5 or less, he had already demonstrated by a direct method. In using this method he found it necessary to prove that a general algebraical equation of the fifth degree could always be reduced to a trinomial form by *real* transformations, which, by Tschirnhausen's (the only method hitherto applied), as often as not, is incapable of being done. By an extension of the principle of Tschirnhausen's method he succeeded in establishing this important algebraical proposition. A very much more important conclusion relating to the representation of every algebraical function (*i. e.* the function that one quantity is of another connected with it by any algebraical equation), under a quasi-explicit form, he believes he can show may be deduced from the transformed Grassmanian construction above alluded to: by quasi-explicit, meaning a form capable of being obtained by the elementary processes of addition, multiplication, change of sign, and reciprocation with that of general form-inversion superadded. Thus Peaucellier's discovery seems likely to throw open a new chapter in the highest summits of Analysis, no less important in the theoretical direction than its numerous applications to the mechanical arts in the direction of practice.

In the lineo-circular or parallel-motion adjustment imagine the connectors to be detached from the angles of the diamond, and joined on to the two sides of the diamond, which meet at the "parallel point," at equal distances from it. Then the motion of that point will no longer be in a straight line, but in a circle.

This method of producing one circular motion from another (which was first given by the speaker in the 'Educational Times') may probably be found to possess important practical advantages over the circulo-circular adjustment of the Peaucellier cell described in the text above.

The speaker exhibited another modification of the Peaucellier cell; like it consisting of six links, but having the property that the sum of the squares of the two arms (instead of their product) remains constant. This he calls a quadratic-binomial extractor.

By means of this cell, mounted with a suitable radius, a perfect lemniscate may be described; and what is very interesting, and flows from this construction (but was first observed by Dr. Henrici), the same curve may be described by means of a binomial-extractor, of a certain kind, reduced to a link-work by the according as the ideal circle, in an arc of which the first-named pole moved, fell short of the fulcrum or contained that point within it; in the limiting case, when it passed through the fulcrum, the path was shown to be neither convex nor concave, but a straight line free from all curvature in either direction. This was further verified mechanically by connecting together at their free poles two perfectly equal and similar mounted cells. If the tendency of either of these was to deviate from the straight path, the tendency of the other would be to deviate in the contrary direction; so that either the pair of mounted cells would become an absolute fixture, or the two would crush or tear each other to pieces; but in the experiment exhibited the pair of mounted cells were seen to move together (as if in happy wedlock), without let or hindrance to each other's motion. The circular motion of the free pole of a single mounted cell in the general case was also verified experimentally, and even more simply than in the rectilinear case, by the addition of a second radial bar, taken of a suitable length, determined by previous mathematical calculation. As a general rule, the total number of bars in a link-work machine must be odd, but here there were eight bars, and yet the combination admitted of being set in free motion,—any one of the eight being, in fact, what may be termed a lazy-bar, and capable of being removed without disturbing the motion, very much in the same way as any one of the four legs of a table may be removed without disturbing the equilibrium.*

The speaker pointed out the important applications of the two kinds of motion above referred to (which he proposed to call the circulo-linear and the circulo-circular respectively) to various constructions in machinery, such as the steam-engine, planing and grinding machines, the construction of maps on the stereographic

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*ablative* method of fixing one of the links: in other words, a perfect lemniscate may be described throughout its complete extent by means of 5-bar motion. Peaucellier refers to, without specifying, a combination, "assez compliquée," of cells (or, as he terms them, compound compasses) by means of which a lemniscate may be traced; whereas, in the method above described the number of links employed is less by a pair than in the single mounted Peaucellier cell.

* Suppose four circles to be given, and that it is proposed to inscribe upon them a quadrilateral whose four sides are given in length.

This is a determinate problem which will in general admit of a definite number of solutions. (The method of correspondence and of bipartite equations founded thereon seeming to indicate thirty-two as the total number of such solutions, some or all of which may be imaginary.) But now the question may be put, "Under what circumstances can the number of such solutions become infinite and the problem undeterminate?" It follows from what is stated in the text above that this may happen (other conditions being satisfied) when two of the circles coincide and the four given lengths are all equal. It remains to be ascertained whether with any new set of conditions a like undeterminateness can be brought about for the case of four circles all distinct. If so, a solution would be obtained of the problem of converting by link-work circular into circular and conceivably (as an extreme case) into linear motion by an arrangement radically distinct from Peaucellier's, and involving the use of three instead of two fixed centres, but with the same number of links.

projection, millwrights' work, laying out of railway curves, dioptric apparatus for lighthouses, ornamental tracery, pendulum suspension to effect motion in a practically exact cycloidal arc, &c., &c., and referred to the use which, as he was informed by the authorities at Woolwich, might have been made of the circulo-circular adjustment in saving several weeks' work, inconvenience, and expense in cutting out the fish-bellied torpedo casings recently constructed in the laboratory department at the Royal Arsenal there, and the use contemplated to be made of the circulo-linear, or perfect parallel motion, for guiding a piston-rod in certain machinery connected with some new apparatus for the ventilation and filtration of the air of the Houses of Parliament, now under course of construction.

He next referred to the unlimited command over the motion of a point furnished by a combination of cells. Returning to the simple Peaucellier cell, its use may be modified in a very remarkable manner by setting free the point of junction of the two connectors (termed, in what precedes, the fulcrum), and fixing one of the poles as a centre of rotation in its place. If now the liberated fulcrum be made to describe any curve, the free pole will describe a curve corresponding to it, according to a certain easily-stateable mathematical law. Imagine the first-named curve to be part of a circle passing through the fixed point—it may be shown that in that case the free pole will describe the inverse of a conic section in respect to a vertex of the conic as the origin of the inversion; consequently, by combining with this cell a second, used as a Reciprocator, we may, mounting with a suitable radius a pair of Peaucellier cells duly adjusted, cause a point to move in a parabola, ellipse, or hyperbola.

The speaker exhibited a combination of this kind, and caused a point to describe portions of an ellipse, a parabola, and of the two branches of a hyperbola in succession; the traversing pole of the first cell, which might be termed the first follower, being seen to describe beautiful nodal cubics (or the inverses of the conics), whilst the free pole of the second cell or second follower described the conics themselves.*

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* The nodal cubics or conic-inverses above described are for the parabola, the common cissoid, and for the ellipse and hyperbola curves which may be termed trans-cissoid and cis-cissoid, or less barbarously and more euphoniously the hyper-cissoid and hypo-cissoid respectively. The common cissoid, as is well known, has a cusp which here coincides with the fulcrum. In the hyper-cissoid this becomes a detached, or, as it is ordinarily termed, a conjugate point, and in the hypo-cissoid a node on the curve, which in this case possesses a loop in addition to an infinite branch. When the first follower moves in this infinite branch, the second follower describes a portion of that branch of the hyperbola in which the fulcrum lies—but of course can never reach the vertex, which coincides with the fulcrum; when the first follower moves in the loop the second follower describes the opposite branch of the hyperbola, and can be made to pass through the vertex of that branch.

The geometrical construction for the common cissoid, or cissoid proper, is well known to be as follows. Imagine a pencil of rays proceeding from one extremity

He next went on to state that by a combination of cells properly proportioned and suitably attached to each other in succession in a manner similar or analogous to that in which simple machines, as for example a number of levers, may be combined to produce a complex

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of a diameter of a circle, and meeting a tangent to the circle drawn at the other extremity. Then if the portion of each ray intercepted between the circle and tangent be shifted along the ray until one point of it coincides with the centre of the pencil, the other point will mark out the cissoid. Now imagine everything to remain as above, with the exception that the tangent is moved parallel to itself and becomes fixed in a new position nearer to or further from the centre of the pencil than it was at first, then the curve marked out becomes the hypo-cissoid or hyper-cissoid respectively, a remark due to Mr. Howard Elphinstone. The smoothness of the motion, and the facility with which the cissoidal curves and the corresponding curves were drawn was matter of general surprise and admiration to the audience. This circumstance, due in part to the skill of Dr. Henrici in choosing the proportions of the parts, ably seconded by the mechanical experience and ingenuity of Mr. Grant, modeller to University College, at the same time served to evince the extraordinary superiority of pure link-work motion, *i. e.* motion due exclusively to the action of radiating bars about centres, over motion effected in whole or in part through the intervention of grooves and slides. It was the analogous superiority enjoyed by circular over linear construction for the purpose of graduating instruments of precision that actuated Mascheroni (the favourite geometer of the first Napoleon) in devising his admirable, most valuable, and most tedious exposition of the geometry of the compass. The superiority in question was still more strongly evinced in the triple-cell combinations employed in the instrument for the extraction of cube-roots and the trisection of an angle.

Clairaut has given a method of constructing an instrument for extracting the roots of an equation by means of linear measurements described in Borgni's 'Traité de Mécanique Appliquée' (volume on 'Machines Imitatives,' p. 226); but the author's method, founded on Peaucellier's discovery, is beyond all comparison superior in the range of algebraical operations which come within its scope, in the simplicity, homogeneity, and smaller number of its parts, in the facility of its application, and the smoothness of the resulting motion. His instrument for solving cubic equations is far less complicated than that of Clairaut for quadratics (which he does not suppose have ever been realized) and infinitely easier of application. For instance, in working his cube-root machine, one point of the instrument is fixed as the zero-point; a second point, called the setter, is drawn out to a division on a scale corresponding to any proposed number; a third point, called the finder, will then automatically place itself over the division on the same scale, corresponding to the cube root of that number. The zero setter and finder points in the calculating linkage are identical (or, as in the transformation scene of a pantomime, may be said to change characters) with the fulcrum, power and weight (or driver and follower), points in the corresponding link-work used as a machine. In Clairaut's and other similar machines the calculations are made by means of measurements made upon curves described by the machines. The author's method is direct and does not involve the use of any such intermediary process.

Returning to the subject, which has led to this digression, it will be noticed that by the method referred to in the text a mounted double reciprocating cell, *i. e.* an apparatus of thirteen links, serves to describe a conic. Peaucellier's method, founded on the combination of what may be termed a collineator or radial protractor, with a mounted reciprocator, involves the use of fifteen links, besides a cross-piece rigidly attached to one of them, and, so far, is less simple, as well as less symmetrical than the author's method: but this must not be supposed to be said in derogation from the merit of the admirable invention of the collineator one, we are able to bring about any mathematical relation that may be desired between the distances of two of the poles of a linkage from a third, and are thus potentially in possession of a universal calculating machine. He exhibited and worked a cube-root extracting machine constructed on this principle, and claimed to have given the first really practical solution of the famous problem proposed by the ancients of the duplication or multiplication of the cube. This machine consisted of a combination of three cells; by changing the modulus of one of the three, he explained that it was also quite easy to solve the cubic equation involved in the analytical solution of the problem of the trisection of the angle; and a working model of an instrument of this kind executed in zinc was exhibited by Professor Henrici after the lecture. He concluded by expressing his great obligations to this gentleman, without whose aid he would have been able to do little more than adumbrate in general terms the results which, thanks to his friend's practical knowledge and skill, he had had the pleasure of exhibiting in a tangible form, and submitting before his audience to the test of actual experiment; and expressed his conviction that Peaucellier's unhoped-for discovery (even if viewed merely on its practical side as a new vital element of mechanism) was destined to produce lasting and important results through innumerable applications to the useful and ornamental arts, and would hand down the name of its inventor to posterity as one of the benefactors of mankind.

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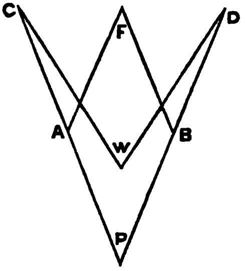
itself, by which Peaucellier has solved the beautiful and most important Kinematical problem of devising a perfect linkage, enjoying the property, that however it is turned about, or drawn in and out, one point of it shall always remain upon or in the direction produced of a physical line rigidly attached to the linkage, but in different positions upon such line. It is believed that a conic-describing instrument (may one say conicograph?) on Peaucellier's plan has not been actually executed, and that a pure link-work for effecting conical motion was witnessed for the first time since the creation of the world in the lecture-room of the Royal Institution on the 23rd of January, 1874. Although it may be presumed that the Peaucellier conicograph would not work so simply as the one exhibited, it possesses a superiority in one respect, *viz.* that the fulcrum on this arrangement lying off the curve at the focus, the part of the curve described may be made to include the vertex of the parabola, which cannot be reached by the other method. It has been thought by competent judges, conversant with practical mechanism, that this (the writer's) method might be applied with advantage to constructing parabolic light-house reflectors; and as these, from the nature of the case, are made *without backs*, consisting of two paraboloids of revolution, situated *dos-à-dos*, having a common focus, at which the source of light is placed, from which the rays stream through the opening upon the surfaces of the two reflectors, the fact of the tracing or cutting or grinding instrument not being able to reach the vertex, would be no disadvantage in this case, since the portion of the surface in the neighbourhood of that point is not required, and, indeed, if formed would have to be subsequently cut away. But it should be added that by a generalized single-mounted cell an approximation to the parabolic form can be attained to a degree of precision far in excess of all practical needs.

## POSTSCRIPT.

In some possibly forthcoming number of 'Nature' a detailed account, which was expected to appear two months ago, will be given, illustrated with the necessary diagrams, of the cube-root extractor and angle-trisector: the materials for this purpose are in the hands of the editor of that journal, and have been entrusted by him to the most competent person to draw them out into form—the writer not feeling within himself the necessary energy for accomplishing this task. He thinks it, however, desirable (indeed almost a moral duty on his part) to supplement those materials by the desultory remarks which follow, in order that some results, which he believes to be important to the progress of mechanical and algebraical science, may be rescued from the chances of total oblivion and virtual annihilation.

The first question which presents itself relates to the square-root extractor. It is a remarkable fact that a cellular system for extracting square roots is much more complicated than what is required for the cube root; and so in general all even-degreed extractors require a more extensive apparatus of link-work than is required for the odd degrees. Such extractions may be performed in all cases by a system consisting of Peaucellier cells exclusively; but the process may be abridged in the case of even degrees by the interpolation of another form of cell, alluded to in a previous foot-note under the name of the quadratic-binomial extractor, which deserves a somewhat more detailed description. It is figured in the diagram below. F A P B is a jointed rhomb or diamond; P C and P D are each doubles of the sides of the rhomb and C W, D W are two equal links. The difference between the squares of C P and C W is the modulus. F P, F W are the arms, and the difference between their squares is equal to the modulus. This is the instrument which, when F is fixed and P moves in a circle passing through W, describes a curve which may be called the Lemniscatoid, having the same general kind of relation to the Lemniscate that the Hypercissoid and Hypocissoid bear to the Cissoid proper. This Lemniscatoid becomes the Lemniscate when a certain simple arithmetical relation subsists between the modulus and diameter of the circle described by P. If A as well as F be fixed, P will move in a circle passing through F, of which C P will be the radius, and consequently the five-bar link-work, consisting of the links C W, C P, D W, D P, F B (centred at F and A), will serve to describe the Lemniscate when the arithmetical relation above referred to subsists between C P and the modulus, *i. e.* between C P and the difference of the squares of C P and C W; consequently, when the lengths C P, C W have a certain simple arithmetical



proportion to each other, W will describe the Lemniscate: this proportion, it will be found, is such that when W comes to F the angle at P is a right angle. So much for the binomial-root extractor: obviously by aid of this kind of linkage when one arm is the tangent of any angle, the other arm may be made equal to the secant, and *vice versâ*. Again, it should be observed that, as in the Peaucellier cell (used as a reciprocator), the arms may be taken as $x$ and $\frac{1}{x}$ by interchanging the fulcrum with one of the poles, *i. e.* reckoning the two arms as the distance between the fulcrum and one pole from the other pole to the arm $x$, the new arm may be made to become $\frac{1}{x} - x$, which may be reciprocated into $\frac{2x}{1-x^2}$, by the use of a second Peaucellier cell. Hence by two Peaucellier cells an arm denoted by tan. $\theta$ may be, so to say, transformed into an arm tan. $2\theta$. Thus we see that we may pass through the following series of transformations

$$\cos.\,\theta,\ \sec.\,\theta,\ \tan.\,\theta,\ \tan.\,2\,\theta,\ \sec.\,2\,\theta,\ \tfrac{1}{2}\cos.\,2\,\theta$$

by means of a P. C., a Q. B. E., a pair of P. C.'s, a Q. B. E., and a P. C.,—*i. e.* by an apparatus containing four Peaucellier cells and two cells of the new kind—making a linkage of six cells or 36 links in all. In other words, by means of such a linkage the arm $x$ may be, so to say, converted into $x^2 - \frac{1}{2}$.

If, therefore, by a Q. B. E. we first convert $x$ into the square root of $x^2 + \frac{1}{2}$ by superadding to this the linkage last named, *i.e.* by a linkage of seven cells or 42 links, $x$ becomes converted into $x^2$. Thus, then, seven cells are required for a squaring or square-root extractor instrument analogous to the cubing or cube-root instrument for which only three cells are required.*

The above investigation leads to a further construction of extraordinary interest, which the speaker is wont to describe as the Kinematical Paradox: every new flight in physics and mathematics, and the same seems equally true of politics, ethics, and philosophy,† is apt to commence with a paradox. Two perfect linkages have been described above, one of six, the other of seven cells. Let these linkages both be constructed simultaneously; they will have two detached points of the one (*viz.* the two extremities of the arm $x$) coincident with two of the other: their union will itself (according to a general principle) form a perfect linkage. In this linkage of 13 cells two points will lie in the same straight line with the original zero point from which the arms are measured, one at the distance $x^2$, the other at the distance $x^2-\frac{1}{2}$ therefrom. Hence there will be two points in this linkage which are disconnected, but in whatever way the other links are drawn in and out, retain an invariable distance from each other! Any other two points of the apparatus may be made to vary their distances from each other, but no force that can be applied at these two points to force them nearer to or separate them further from each other can be of any effect. There is no immediate rigid connection between them, and yet they are as good as rigidly connected. Imagine now that they become connected by a material link: the linkage will not be a fixture, but a perfect linkage as before, consisting, however, of an odd number, *viz.* 79 links; any one of these may be regarded as a lazy-bar, and may be removed without affecting the motion of which the apparatus is susceptible. Returning to the original state of things, where there are 13 cells, if we fix the two points of invariable distance the instrument will not become a fixture (as would be the case if any two other disconnected points in it were fixed), but a free link-work with a superfluous or lazy-bar, represented by any of the links at will; for by fixing these particular two points, not *four*, but only *three* degrees of liberty are abstracted. By fixing one of them two such degrèes are taken away; but as the other is then not free, but compelled to move in a circle, fixing *it* takes away only one additional degree of liberty of motion.

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* The much simpler scheme for converting $x$ into $x^3$, which explains the principle of the cube root machine, is as follows:—

First conversion, $x - \frac{1}{x}$, *i.e.* $\frac{x^2-1}{x}$.

Second conversion, $\frac{x}{x^2-1} - \frac{1}{x}$, *i.e.* $\frac{1}{x^3-x}$.

Third conversion, $(x^3 - x) + x$, *i.e.* $x^3$.

For the trisection of the angle it is necessary to solve Kinematically the equation between cos. $3\,\theta$ and cos. $\theta$, to effect which it is only necessary to replace the third conversion above by $4\,(x^3 - x) + x$, *i.e.* $4\,x^3 - 3\,x$.

† As ex. gr. Cramer's paradox (the foundation of the highest modern geometry) the *ποὺ στῶ* of Archimedes and the hydrostatic paradox, "The king can do no wrong" "It is better to suffer than to do wrong" "All proof is reducible to

By this link-work of 78 bars (one superogatory) a remarkable Kinematical problem has been solved (and it is probably the simplest solution of which it admits), which may be stated as follows:—"Required to construct a link-work fixed or centred at two of its points, such that (when the machine is set in motion) some other point or points therein shall be compelled to move in the line of centres."

There are some similar questions to this, which ought, in a strict logical order, to have preceded it, which we may now take into consideration. By a single mounted Peaucellier cell fixed at two centres, one point is made to move perpendicular to the line of centres. Suppose now it were required to devise a link-work such that a point should move parallel to such line.

The motion perpendicular to the line of centres is due to the fact that by the Peaucellier cell the radius vector C cos. $\theta$ is transformed

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syllogisms, and the syllogism can prove nothing" "A heavy body begins to fall with no velocity." The Kantian antinomies. Helmholtz's vortices. A variable function which never varies, *i.e.* an Invariant as distinguished from a Constant.

into C sec. $\theta$; in like manner to get the parallel direction a means must be found of passing from the cosine to the cosecant. Now although a single cell serves to change the tangent into the secant, *or vice versâ*, and consequently a single *imaginary* cell will serve to change the cosine into the sine (which of course could then be immediately Peaucellierized into the cosecant), he is not aware of any direct real process simpler than that about to be stated by which this can be effected. His actual law of deduction is as follows: Cosine; secant; tangent; cotangent; cosecant, involving the use of two Peaucellier cells and two quadratic-binomial extractors.

With one cell more, *i.e.* with five in all, the cosine becomes converted into the sine, and consequently by introducing a pantigraphic cell cos. $\theta$ may be converted into cos. $(\theta + a)$, and this reciprocated into sec. $(\theta + a)$. Thus it seems (at all events after the present method) that four cells are required to obtain by link-work rectilinear motion parallel to the line of centres, and seven cells to convert it into motion oblique to the line of centres; or taking into account the mounting radius 7, 25, 43 links are required to obtain motions respectively perpendicular, parallel, and oblique to that line. In the Kinematical Paradox it will have been seen that there are 13 cells employed, *i.e.* 78 links, of which any one is liable to removal at will, so that for motion in the very line of centres 77 links are requisite. Consider this system in its entirety. In a straight line with the two fixed points there will be 13 other medial points; and two parallel ranks on both sides, each also containing 13 points. The whole apparatus admits of being moved with a sort of see-saw motion backwards and forwards; and it may assist the imagination of the reader if he will conceive such an instrument armed with 13 picks in the line of centres, each at work to remove the asphalt of a pavement under repair; an idea suggested by a member or visitor at a soirée of the Amateur Mechanical Society of London, of which the ingenious and accomplished "Senior Member for Greenwich" acts as honorary secretary. Or we might describe the Kinematical Paradox as a kind of compound saw. If the "two points of invariable distance" be set free, and some other of the medial points be fixed as a fulcrum, the instrument may be used like Peaucellier's second invention referred to in a previous foot-note as a radial protractor to change the curve

$$\rho = \text{a given function of } \theta$$

into the curve

$$\rho + c = \text{the same function of } \theta;$$

as, for instance, to pass from the circle to the limaçon of Pascal, or from a straight line to a conchoid. For while one of the two points of constant distance described any curve, the other would describe the same curve with all its radii vectores reckoned from the fixed point lengthened or shortened by a constant quantity. The Kinematical Paradox ought not to be regarded in the light of a mere luxury of speculation; it serves to represent a constant as a Kinematical function of the independent variable (corresponding to the use of the zero power of $x$ to represent unity in algebra), without which the general analytical theory of linkages, and the very important theory of algebraical functions founded thereon, would fall to the ground, or rather be incapable of being constructed.

It would be difficult to quote any other discovery which opens out such vast and varied horizons as this of Peaucellier—in one direction, as has been shown, descending to the wants of the workshop, the simplification of the steam-engine, the revolutionizing of the millwright's trade, the amelioration of garden-pumps, and other domestic conveniences (the sun of science glorifies all it shines upon), and in the other soaring to the sublimest heights of the most advanced doctrines of modern analysis, lending aid to, and throwing light from a totally unsuspected quarter on the researches of such men as Abel, Rieman, Clebsch, Grassman, and Cayley. Its head towers above the clouds, while its feet plunge into the bowels of the earth.

Prophetic and well-timed were the parting words to the speaker of the illustrious Tchebicheff: "Take to Kinematics, it will repay you; it is more fecund than geometry; it adds a fourth dimension to space." So also said Lagrange.

In the course of the foregoing exposition, incidental reference has been made to the addition of perfect linkages to each other.* This gives rise to the important distinction of all perfect linkages into prime and composite—prime ones being such as can be resolved into the sum of two others, and composite those for which no two such components can be found. As an example of one kind, imagine an octagon with its four pairs of opposite angles (or, which will do as well, its four pairs of opposite sides) connected by links. There will then be 12 links and 16 joints; and since $3 \times 12 - 2 \cdot 16 = 4$, the linkage will be perfect. Such a linkage is prime, for it will be found impossible to resolve it into two others. Whereas, every cell previously described is capable of being formed by the successive accretions of single pairs of links, thereby justifying in a new and specialized sense the title of Compound Compass, used by Peaucellier to designate his cell. Moreover, cells belong to a very special class of compound linkages, those namely which by successive processes of decomposition can eventually be reduced to depend on sets of link-pairs, and which may accordingly be termed Dyadisms. Dyadisms, again, require to be classed according to their order. A dyadism of the first order is one that can be obtained by successive additions of single duads at a time. A dyadism of the second order is one that can be formed by successive additions of single dyadisms of the first order at a time, and so on; and it is very essential to notice that the addition together of two dyadisms of a given order will not in general be a dyadism of the same order. Thus we see that a pure tactical theory of colligation underlies the subject of linkages, a theory of the same nature as that which is known to underlie the doctrine of crystallography and polyhedra; and as that which, under the name of ramification (proposed by the speaker), gives the clearest notion of the modern chemical doctrine of the atom-groupings of the hydrocarbons, and in a manner supplies an *à priori* ground for the formula of the saturated hydro-carbons $C_n \cdot H_{2n+2}$ which, for the simpler case of the hydro-borons (if such series existed), would become $C_n B_{n+2}$

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* *Viz.* by pivotting together two disconnected points of the one with two disconnected points of the other, each with each. The sum of two perfect linkages so connected will satisfy the same numerical linear equation between joints and links as its two constituents, and thus will itself constitute a perfect linkage.

It may be shown that every ramification may be subjected to a process of reduction, (a sort of divulsion process, the number of steps of which fixes its genus, or order) which leads eventually to a single intrinsic centre or a pair of intrinsic centres, and consequently may be referred to one or the other of two great classes of forms which may be termed central and axial respectively; and it seems only reasonable to anticipate that the physical properties of such chemical compounds as the hydrocarbons will eventually be found to correspond to this distinction between their representative ramifications; and that they will accordingly range themselves under one or the other of two great families distinguished by properties at least as important and specific as those which serve to distinguish the crystalloidal and colloidal states of matter. The theory of ramification is one of pure colligation, for it takes no account of magnitude or position; geometrical lines are used, but have no more real bearing on the matter than those employed in genealogical tables have in explaining the laws of procreation.

The sphere within which any theory of colligation works is not spatial but logical—such theory is concerned exclusively with the necessary laws of antecedence and consequence, or in one word of *connection* in the abstract, or in other terms is a development of the doctrine of the compound parenthesis. M. Camille Jordan, independently of and anteriorly to the author, discovered and published in a memoir, the title of which would never suggest the notion of ramification, the existence of the intrinsic centre and centres here referred to—without having any suspicion of its bearing on modern chemical doctrine. He has moreover discovered the existence of another kind of intrinsic centre of ramification which was unknown to the author of these lines.

A ramification, it ought to be added, is a rootless tree, *i.e.* one in which the root only ranks the same as the terminal of a branch, and saturated hydrocarbons are typified by ramifications in which every joint is trifurcated, meaning thereby that in tracing the wood outwards from any terminal assumed as the root, it splits and splits again, so that trifurcation takes place at each joint, or in other words, *four* lines radiate out from each joint;* the joints are supposed to adumbrate the carbon atoms and the terminal points the hydrogens.

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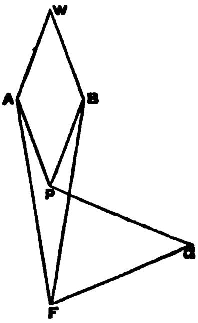
* Observe that if there were *no* splitting, as in a bamboo cane, *two* lines would issue from each joint.

To conclude, as he has begun, with the principal personage of his story, the author thinks it will be useful to several of his readers to have before their eyes the figure which contains the property of the admirable linkage which lies at the root of Peaucellier's conicograph.



In the given figure A P B W is a rhomb. P A is equal to P B, G P to G F, and G′ is a point lying on F G, or F G produced such that F G′ W is a right angle. Then, however the links are moved about, the motion of W *relative* to F G will be always perpendicular to F G, from which it follows that F G′ W will always continue to be a right angle, and consequently an upright piece attached at G′ perpendicular to F G will always continue to point to W. When W is fixed, the instrument serves as a radial protractor. One point of the upright can describe any curve, and any other point a radial protraction (or retraction) of that curve. When one point of the upright perpendicular is fixed, the combination becomes ideally equivalent to a revolving slot, in which W is free to traverse. The inverse of a conic in respect to a focus (*i.e.* the Limaçon of Pascal) is a protraction or retraction of the circle. Hence the use of the instrument for describing conics.

In the above linkage let a pair of equal links G P, G W be *substituted* for the pair G P, G F. It is easy to prove that if O be the intersection of the diagonals of the rhomb, G O and F O will then be at right angles to each other, and the sum of their squares will be a constant. If now any one link of the rhomb is transferred parallel to itself so as to pass through O, and is jointed on to the sides at the points where it meets them, and O is fixed, and F made to move in a circle containing O, the path of G will be the *inverse in respect to* O *of a conic* of which O is the centre, so that by the aid of a radius and a reciprocator in addition to the transformed linkage above described, a point may be made to move in any conic round its *centre* as a fixed point.* This is rather a simpler construction than Peaucellier's for motion in a conic round the *focus* as a fixed point, for the number of links is no greater, and the ungainly cross-piece disappears. Moreover, it possesses all the advantages of Peaucellier's method arising from the fulcrum lying off the curve to be described. Finally, as regards the most general motion that can be produced by a Peaucellier-mounted cell in its generalized form, if F be the junction of two links on which F A, F B are two equal segments, and F C, F D two other equal segments, and P A, P B and W C, W D be two pairs of equal links in the same plane with the first pair, such combination of three pairs is the generalized form of cell in question. In applying it to draw curves, F may be fixed, and a mounting radius of any length attached to P or W, or P or W may be fixed, and the mounting radius attached to W or P, or P or W be fixed, and the mounting radius attached to F. In a resumé of this general kind it would be out of place to enter into a discussion of the forms thus generated.*

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* It follows as a particular case of the above, that an apparatus of nine links moving round two fixed centres will serve to generate motion in a circle whose centre is in a right line drawn through one of the given two, perpendicular to the line joining it to the other.

[J. J. S.]

ATHENÆUM CLUB, AND 60, MADDOX STREET, W.

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* It is too late to make any change in the many places where the term perfect linkage appears in the text, but the author regrets to have used the word *perfect* when *complete* would have expressed the meaning more clearly, and suggests this change of nomenclature to any writer who may hereafter have occasion to employ the term—besides being better in itself, it comes nearer to Peaucellier's "système de tiges à liason complète;" two words (and those much more expressive) supplying the place of six. The existence of such words as surplusages, curtilage, equipage, assemblage, and many similar ones in the English language, appears quite sufficient to justify the innovation in the use of the final syllable in linkage. A question of great interest remains over, *viz.* "how to extend the above inquiry to linkages in space;" any two links being supposed free to move by means of universal joints in all directions round each other. As regards surfaces of revolution, the solution of the problem is virtually contained in the theory of plane linkages, and consequently as a plane may be regarded as a surface of revolution, the difficulty does not begin to be felt until the problem of producing motion in an ellipsoid or other surfaces of the second order, by means of solid link-work, comes under consideration. It seems to be a problem well worthy of being investigated and thought out, especially for the sake of its analytical consequences and the light it might be expected to throw upon the theory of algebraical functions of two variables.

## WEEKLY EVENING MEETING,

Friday, January 30, 1874.

George Busk, Esq. F.R.S. Treasurer and Vice-President,
in the Chair.

Sir Julius Benedict

## *On Weber and his Times.*

[Abstract.]

**Weber's** father, Franz Anton, Baron von Weber, born in 1734, was a most eccentric character; an ex-lieutenant in the Guard of the Elector of the Palatinate, ex-financial Counsellor and District Judge to the Bishop of Lubeck and Eutin. At all times attached in a wayward manner to music and the drama, he at last, under an assumed name, started as director of a wandering troop of actors. After having previously squandered the fortune of his first wife, who died of a broken heart, Franz Anton, in his fiftieth year, and father of five children, was married again at Vienna to Genovefa de Brenner, a fair, delicate girl of sixteen. Unsuccessful in the Austrian capital, he returned to Eutin, and there became the town musician. It was under these circumstances, on the 18th Dec., 1786, Carl Maria von Weber was born, a sickly, weak child, suffering from a disease of the hip-bone, which was subsequently the cause of his permanent lameness.

His father, whose brother's daughter had married Mozart, had long determined to have a prodigy in his own family, and his poor boy, who could not walk till his fourth year, was taught to put his tiny fingers on the piano and to sing almost before he was able to speak. Baron Weber visited various towns in Germany with his strolling company, his wife and child following till they reached Salzburg. There poor Genovefa breathed her last, before her darling son had reached his twelfth year, who meanwhile suffered much both morally and physically. . . . With an incomplete education, and not even conversant with the fundamental rules of harmony, he was forced to write all kinds of music, amongst other things an opera, 'The Power of Love and Wine.' . . .

At Carlsbad the manager of the theatre, Chevalier von Steinsberg, conceived a great interest for the young genius, and even trusted to his hands the libretto of an opera written by himself, 'The Dumb Girl of the Forest,' which was first performed at Chemnitz, and afterwards, on the 24th November, 1800, at Freiberg in Saxony, with little

or no effect, when Franz Anton's disappointment was terrible. A few concerts in the provinces enabled father and son to return to Salzburg, where Carl resumed his studies with Michael Haydn, and formed a lasting friendship with his fellow-pupil, Sigismund Neukomm, known in England as the composer of various oratorios and of the favourite song 'The Sea.'

Here, in complete retirement, a comic opera, 'Peter Schmoll and his Neighbours,' was written, which, at its production at Augsburg in 1803, left no trace of either failure or triumph. At the end of that year the young composer is heard of at Vienna, where his father selected, in preference to a sound musician for his teacher, the fashionable composer and organist of the period, Abbé Vogler, through whose recommendation Weber obtained the appointment of conductor of the opera at Breslau at the age of eighteen. Here he acquired his perfect knowledge of the stage, and advanced to the highest degree of proficiency on the piano. A dreadful accident now pretty nearly put an end to his career. He was laid up for two months, and his enemies availed themselves of his temporary illness to render his position impossible to be held. Fortunately he found a happy asylum in the house of Duke Eugen Friedrich of Würtemberg, at Carlsruhe in Silesia, where he passed a few peaceful months, conducting a small but excellent orchestra. But the disastrous war with France in 1806 began to tell heavily on the social state of Germany; yet, though obliged to give up his establishment, Duke Eugen did not forget his protégé, but recommended him warmly to his brothers, the King and Duke Ludwig of Würtemberg, and Weber became private secretary to the latter.

When at Stuttgardt he made acquaintance with the sculptor Danneker, Matthison the poet, Louis Spohr, and Danzi, then the clever conductor of the opera. Through an unpardonable indiscretion on the part of his father, Weber was compelled to leave Würtemberg, and went to Mannheim. There, in the house of his namesake, the celebrated theorician, Gottfried Weber, he found an asylum for his father, to whom, though suffering from the mischief he had done him, he was attached with a filial devotion, and resumed his studies with Vogler.

From that period the real development of Weber's genius may be said to have begun. Another of Vogler's pupils, Jacob Mayer Beer (since Meyerbeer), was the son of one of the wealthiest bankers in Berlin. From his boyhood he evinced undeniable talent for music, and, forgetting his millions, took to it in right earnest, being already, then in his seventeenth year, one of the most eminent pianistes of his time. This led Carl Maria to display a restless activity. His first pianoforte concerto, a great number of his most popular Lieder, six sonatas for piano and violin, and his operetta, 'Abu Hassan,' were all produced in the same year. On the 17th September, 1810, his opera, 'Sylvana,' was first performed at Frankfort; but a balloon ascent, by the famous Madame Blanchard, so absorbed the public that the theatre was nearly empty. The work itself, then only a feeble "riffacimento" of his first opera, 'The Dumb Girl of the Forest,' though applauded by the scanty audience, achieved merely a *succès d'estime.* The principal part was performed by a charming young artiste, then in her eighteenth year, Caroline Brandt, who was destined to become Weber's partner for life.

At the beginning of 1811 Carl Maria began his great artistic journey through Germany and Switzerland, first alone, then in company with Baerman, an eminent virtuose on the clarinet. On the 20th February, 1812, Weber took up his abode at Berlin, in the princely house of Meyerbeer's father, where he was received as a son and brother. It was the first time he remained long enough in the great North German city to allow its peculiar intellectual and artistic tendencies to exercise any permanent effect on his own organization. He succeeded in having his opera, 'Sylvana,' brought out at the Opera House. It was received apparently with much favour, but neither then, nor at its reproduction in 1814, did the work outlive more than a few representations, though it contains much to be preserved from oblivion. During the struggle relative to the production of 'Sylvana,' Carl Maria received from Gottfried Weber the news of his father's death, which affected him deeply. . . . .

Setting out again on his wanderings he arrived at Prague, where he was appointed Musical Director of the theatre, and being charged by the Impresario to provide artists for the season, he secured as one of the vocalists Caroline Brandt, who contributed greatly to his first success. He eventually declared his attachment to her, and was accepted.

Arrived at Berlin, he suddenly found himself in an atmosphere of agitation and excitement pregnant with aspirations—the vastness and unanimity of which were new to him. The German people, by its own will, its own power, and its own sacrifices, had subdued its great oppressor; and it stood, after its victory, like a young lion that had first felt its strength. There was but one feeling in all hearts, the feeling of self-won victory, power, and freedom. Weber carried all his impressions with him to Gräfen-Tonna, an old castle in Thüringen, where, *en route* to Prague, he stopped at the wish of his friend and admirer, the Duke of Saxe-Gotha, and wrote the two inspirations which were to thunder throughout Germany, 'Lutzow's Wild Hunt,' and the 'Song of the Sword.'

On Christmas day, 1816, he received the long-expected engagement as Capellmeister to the King of Saxony, with a very modest salary, and now began the period of combined fame and suffering which did not terminate until his death in 1826. Here his wedding took place on the 4th of November, 1817.

The King, Frederick August, who had a horror of any innovation, only yielded reluctantly to the representations made on all sides, that in regenerating Germany a German opera was requisite, Italian opera only being recognized by the crowned heads of the German Confederation. . . . . Count Einsiedel, the King's "Alter Ego," a haughty, heartless courtier of the old school, conceived a dislike to Weber at his first interview. He delighted in worrying the young Capellmeister, whilst he showered his favours upon his colleague and rival, Morlacchi, a composer of the third order, but a wily and unprincipled intriguer.

Weber's creative powers were very slightly tasked during all this stormy period at Dresden, and it was only on the 23rd February, 1817, when he received from Frederick Kind the first act of an opera, 'The Hunter's Bride,' that, when reading it over, as it were a spring of melody bubbled up in him. Not until four months later did he write a note of it; but his mind grasped the subject, and the complete work stood before him long before he gave it a substantial form. There is not a single piece of music in it that he did not turn over twenty times in his mind, until he so felt it that he could write it down in a firm, clear hand, almost without altering a note. He may be said, then, to have been always composing. The world appeared to him a world of tones; colour, form, space, time, were transformed by a mysterious process of his inward man into sounds. But happy as might be these ideas, Weber was slow at writing them down.

Many other works, including his first and second Mass, his cantata for the Golden Wedding of the King, his wonderful 'Volkslieder,' a charming set of eight pieces for two performers on the piano, and his popular 'Invitation à la Valse,' as well as the last of his grand sonatas, belong to this period.

The interesting sketch, 'Invitation à la Valse,' has its historical value. It marks the transition to modern dance music. The valse had been previously a sort of mere animated minuet, but Weber threw a fiery allegro into the dance. The simple, almost childish sentimentality of the beginning of the century, with its pale, insipid style of dancing, had sighed itself out. Weber bestowed upon his music a fine chivalric tone, mixed with human passions and feelings. Dignity and simplicity had both found their representative music in the ballroom, then why should not love? He was the founder of dance music expressive of deep feeling, and of the school to which Strauss afterwards belonged. He showed how profoundly he was imbued with the spirit of his age, and sarabandes, gavottes, and minuets were banished through his influence for ever.

Meanwhile 'The Hunter's Bride' was again rising to the surface of Weber's artistic life. Notwithstanding a serious illness which had laid him low, the death of his dearly-beloved first child and numberless other troubles, the news that the theatre would probably be opened before the close of the season 1820 spurred him on to fresh exertions, and on the 13th of May of that year the last note of the opera was written. The work was accepted after a change of the title from 'The Hunter's Bride' to the 'Freyschütz.' Scarcely was this accomplished when another engrossed all his time. The musical drama of 'Preciosa,' written by the eminent actor Wolff, was to be produced at the Grand Opera House at Berlin immediately. The subject interested the fertile composer so much that he finished the work in less than two months.

At the beginning of February, 1821, it was my happy lot to be accepted as Weber's pupil. I shall never forget the impression of my first meeting with him. Ascending the by no means easy staircase which led to his modest home on the third story of a house in the old market-place, I found him sitting at his desk, and occupied with the pianoforte arrangement of his 'Freyschütz.' The dire disease which but too soon was to carry him off had already made its mark on his noble features: the projecting cheek-bones, the general emaciation, told their sad tale; but in his clear blue eyes, which were but too often concealed by spectacles; in his mighty forehead, fringed by a few straggling locks; in the sweet expression of his mouth, in the very tone of his weak but melodious voice, there was a magic power which attracted irresistibly all who had the privilege of approaching him. He received me with the utmost kindness, and though overwhelmed with double duties, as in the temporary absence of Morlacchi he conducted both the German and the Italian operas, he found time to give me daily lessons for a considerable period.

His previous operas had only achieved a moderate success, and Weber felt that his new work would either raise him to a high pinnacle or carry him to the grave with scorn and mockery. When I joined him at Berlin, on the 24th of May, the rehearsals were in full swing. Weber had the whole orchestra under his control. Not a wrong note, not a false accent escaped his searching ear, and though in appearance not demonstrative nor excited, in his direction he infused his spirit and energy on all the performers.

The first performance was fixed for the 18th of June, the anniversary of the battle of Waterloo. On the preceding morning the general rehearsal took place, and was successful beyond expectation, but the accessories, machinery, &c., were still wofully deficient.

[Sir Julius described the very great success and commented on various details of the first performance.]

However, the profound and genuine enthusiasm of the public was not shared by the press of the day. Zelter, in writing to Goëthe, treated the subject with derision, and finished by saying that out of a *small* nothing the composer had created a *colossal* nothing. Tieck spoke of the 'Freyschütz' as the most unmusical uproar ever heard upon the stage. Spohr wrote: "As I never had a great opinion of Weber's talent as a composer, I wanted to hear this opera to discover the secret of its wonderful success; but this riddle was by no means solved, and I can only explain it by the gift possessed by Weber to write for the general masses." Weber resented the sting of those harsh and unjust criticisms very much. He had been working earnestly, enthusiastically for what he considered a real advance in art, and it was painful to him in the extreme to be so entirely misunderstood, chiefly by those on whose sympathy and encouragement he had reckoned so much. . . . He had observed in his long career as conductor, that the form of operas sanctioned for so many years did not entirely answer the requirements of the age. Each piece in the lyric drama, belonging to the Italian *répertoire*, whether an aria, a duet, or a *morceau d'ensemble*, was perfect in itself as a musical composition, and might be performed without scenic effects, and without the aid of the vocalists, choruses, &c. The result was that these elaborate pieces became cloying by their sameness, nay, by their very finish and perfection. There was no attempt at individuality. Not so with Weber. His first aim was to endow each of his operatic works with a distinct colour of nationality. For instance, the contrast between the simple 'Hunter's Bride,' with her surroundings, and 'Euryanthe,' with the stately French court, could not be more striking. But he was not satisfied with this general result; he made each character stand out in bold relief. . . . With his memory the foundation of the Romantic school will always be associated. Meyerbeer's 'Robert le Diable' would not have been written but for 'Freyschütz'; and Richard Wagner's 'Tannhaüser' and 'Lohengrin' can be traced to 'Euryanthe.' In his pianoforte sonatas, Weber comes nearest to Beethoven; in his Masses he approaches Haydn; and his 'Concert-stück' is, as it were, a pioneer of Mendelssohn's concertos. He may also, with Schubert, be called the father of the German Lied; and thus, in his short but glorious career, embracing almost every branch of music, he gave an impulse to his art of which the beneficial consequences can never be overrated. It is, however, in the hearts of the people he found his truest acknowledgment, and in them he will live for ever.

On his return to Dresden, he found his little nest on the 'Alt Markt' transformed into a veritable garden of roses through the affection of his friends; but not the slightest notice of his well-earned triumph was taken by his colleagues, his superiors, his theatrical subjects, or the public. The only difference he found was a fearful increase of professional jealousy. Things went on in their despairing monotony. . . .

The unexampled success of Berlin was completely thrown into the shade by the frantic reception of the 'Freyschütz' at Vienna in 1822. Every theatre in Germany followed in the track of the two capitals; and everywhere without exception was the work received with acclamation. At Paris Castil-Blaze made a fortune with 'Robin des Bois,' a disgraceful *pasticcio*. At London the Lyceum and Covent Garden Theatre produced the opera almost simultaneously, with the garbled versions and interpolations of Cooke and Bishop, to crammed and enthusiastic houses; yet poor Weber only received a small compensation from W. Hawes, and not a penny from France. The total receipts, including pianoforte score, and copyrights from sixty theatres, did not amount to 700*l.*, whilst Mr. Lecocq, the composer of 'La Fille de Mme. Angot,' has earned to this day nearly 6000*l.* for authors and copyrights, and Verdi's 'Aïda' brought its author more than 10,000*l.*

Offers of engagement from all sides poured now upon Weber. Barbaja, the former billiard marker, and now Impresario of the three theatres, Vienna, Milan, and Naples, gave him the choice of either for a new opera. The direction of the grand opera of the Académie Royale at Paris left the selection both of subject and time to Weber's option. Copenhagen and Stockholm were also among the postulants. He decided for Vienna, but before concluding a binding engagement there, the too conscientious maestro had offered to complete a comic opera, 'The Three Pintos,' for the Dresden stage, and as homage to the king, which was declined.

The chief difficulty now arising was how to find an appropriate libretto. F. Kind, his clever literary colleague, had taken offence that his share in the success of the 'Freyschütz' was never sufficiently acknowledged by the press, and persistently declined furnishing another subject. Weber, who had felt dreadfully annoyed at the sneering remarks that after all 'Freyschütz' was only a drama with music, and not a regular opera, resolved to compose a grand work, without spoken dialogue, in the true heroic style, in order to give the lie to his galling detractors. Frau Helmina von Chezy, a middle-aged lady, who had acquired a certain celebrity by her readings of hyper-sentimental songs and by a fatal facility for writing verses, was chosen to furnish the canvas for Carl Maria's grand musical picture. A more inane, uninteresting, antidramatic patchwork was never offered to a composer than this miserable burlesque of Shakespeare's 'Cymbeline,' mixed with absurd ingredients of her own invention. The modifications proposed by Tieck and Holtei, dramatic authors of great experience, were unfortunately rejected, and with an almost unnatural activity Weber went to work, being elated by the birth of his eldest son, Max, on the 28th April, 1822.

In his Tusculum at Hosterwitz, where he was very often gladdened by the visits of Jean Paul Richter, Tieck, and Wilhelm Müller (the poet of Greece and father of Professor Max Müller), I had the happiness to spend one of the most interesting periods of my life with him and his family. Watching the progress of his opera, 'Euryanthe,' from the first note to its completion, I had the best opportunity of observing his system of composing. Many a time might he be seen early in the morning, some closely-written pages in his hand, which he stood still and read, and then wandered on, muttering to himself, through forest and glen. He was learning by heart the words of 'Euryanthe,' which he studied until he made them a portion of himself, his own creation, as it were. His genius would sometimes long lay dormant, during his frequent repetition of the words, and then the idea of a whole musical piece would flash into his mind like the bursting of light into darkness. It would then remain there uneffaced, gradually assuming a perfect shape, and not till this process was attained would he put it down on paper. His first transcriptions were generally penned on the return from his solitary walks. He then noted down the voices fully, and only marked here and there the harmonies or the places where particular instruments were to be introduced. Sometimes he indicated by signs, known only to himself, his most characteristic orchestral effects; then he would play to his wife or to me from these incomplete sketches the most striking pieces of the opera, invariably in the form they afterwards maintained.

The whole was already so firmly stereotyped upon his brain, that his instrumentation was little more than the labour of a copyist; and the notes flowed to his pen with the marks of all the shading of expression, as if copperplated on the paper. By this peculiar mental process the enormous quantity of work which he was able to accomplish in an almost incredibly brief period can be explained. The scoring of the opera of 'Euryanthe' from his sketches occupied only sixty days.

In the afternoon of the 21st September, 1823, Weber arrived at Vienna, accompanied by me.

Great changes had taken place since his last visit ten years before. Beethoven now reigned supreme. A new light had arisen in Franz Schubert, whose songs, 'Erl-King' and 'Margaret,' and whose opera, 'Rosamund,' foreshadowed his future celebrity, and Rossini had brought an entire bevy of song-birds such as had never before enchanted the ears of the fickle and impressionable Viennese.

The rehearsal of the opera began almost immediately. All the artists had taken to their parts with great zeal—none more so than the charming Henriette Sontag, then in her eighteenth year, to whom the principal part was allotted. The choruses went admirably—the orchestra was almost perfect.

On the surface all seemed smiling and prosperous, but the heart of the public was not with him as at Berlin in 1821. Artists found fault with the work. Franz Schubert spoke in a manner which proved that there was no sympathy between these eminent composers. Advantage was also taken of the alleged feud with Beethoven, brought about by a foolish satire against the 'Eroica,' written more than twelve years previously, and which Weber never ceased regretting. Everything was done to foster that hostile feeling, but the mighty Ludwig was above small talk and mischievous gossip, and most warmly received Weber at an interview desired by himself.

At the performance on the 25th of October the outer demonstrations were certainly far noisier, the demands for encores more frequent, than on the first performance of 'Freyschütz,' but not even the gorgeous '*mise en scène*," the admirable interpretation of the work by chorus and orchestra, under the master's own direction, and the precious gems of melody, so lavishly spread over the whole, could save the opera from the fatal and heavy weight of its plot.

The performance had lasted for five hours, and the Italian clique said that 'Euryanthe' should be called 'Ennuyante,' and that as Weber was writing for eternity, his opera must be eternally long. The author himself used the pruning knife mercilessly, but the public had been unfavourably impressed, and on the fourth performance, the evening of his departure from Vienna, the theatre was empty. He had scarcely left when the magnificent creation, now generally acknowledged as his masterpiece, was cut almost to shreds by Herr Kreutzer, and withdrawn after the comparatively insignificant number of twenty representations.

A pause now ensued in the exercise of Weber's creative powers. During fourteen months after the production of 'Euryanthe,' not a melody was awakened in his soul, not a scrap of song was noted down. His inactivity became so habitual to him, his antipathy to work so great, that he shrunk from the consciousness of this state of mind with a kind of terror. His increasing weakness rendered it imperative for him to diminish all his labours not immediately connected with his official duties, and thus I was compelled to part with my beloved friend and master. . . . .

He then fell a prey to distressing presentiments for which he could not account. The feeling was more than ever deeply rooted in him that the end of his earthly existence was at hand, and his ardent wish to provide for his increasing family became almost painful. Though most unwilling to increase Weber's anxieties, his physician, Hedenus, at last yielded to his ceaseless entreaties. "If you will abandon your every day's drudgery," he said, "forget music and composition, and seek relief in a southern climate; though you may not, even then, recover your health entirely, the disease being too deeply rooted, you may still live some years." "And if not?" replied Weber. "Then it is only a question of a few months," was the disheartening answer of the old doctor.

Weber had been to Marienbad during the month of August, and on his return entered upon the last phase of his life. In the beginning of September, 1824, Charles Kemble wrote him a most flattering letter from London. Being the lessee of Covent Garden, he was anxious to give additional *éclat* to his direction by a new opera composed by the author of 'Freyschütz' (only a few weeks earlier a similar proposal had been made by the Grand Opera at Paris). Weber, who had always felt a strong sympathy for England and for the illustrious family represented by Kemble, accepted the London offer. 'Faust' or 'Oberon' (taken from Wieland's poem) would have been equally welcome to the lessee of Covent Garden; but the composer having chosen the subject of his former friend and admirer, the tact, experience, and talent of Planché were immediately put in requisition, and on the 30th December Weber received the first act of the libretto.

The poor sufferer, struggling with death, began only then to study the English language, which by unremitting exertions he acquired in a short time to an astonishing degree. . . .

How in his state of prostration he could find the freshest, perhaps the most original melodies his fertile pen ever produced, seems almost marvellous. There is a perfume of oriental roses in the song, 'Araby, dear Araby!' and the mermaid's song, 'O 'tis pleasant to float!' will be as fascinating fifty years hence as it was fifty years ago.

The whole opera, excepting the overture, was completed on the 25th January, 1826, and on the 7th of February he started on his ill-fated journey. That there was not a moment to spare, that he must reckon with a miser's avarice every pulse of the life so quickly ebbing from him, and that each must be used to the advantage of wife and children, was his uppermost conviction. His wife manœuvred in secret for his friends to induce him to renounce his fearful journey, when all her own affectionate efforts to this intent had failed; but his answer was the same, "Whether I undertake this journey or not, it is all one; within a year I am a dead man; but my children will have bread."

Fürstenau, a distinguished flute-player, a member of the Royal Chapel, and devoted to him, was his travelling companion. The parting was most distressing; still the novelty of the journey in France acted as a diversion, and on the 25th he arrived at Paris. Though resolving at first to remain incognito, he could not resist the desire to make the personal acquaintance of the great masters, Cherubini, Rossini, and Auber, by whom he was received in the most friendly and courteous manner. Each visit to the theatre brought an ovation, gratifying to his feelings, but still more undermining his shattered frame.

On a cold, unfriendly March morning, Weber left Paris with Fürstenau. At Calais the consequences of his exertions at Paris well-nigh ended his life. For a long time he gasped for breath, but by force of will he rallied at last; and on arriving in London accepted the hospitable invitation of the late Sir George Smart, and the kindness and sympathy there bestowed on him were the solace of his last days.

He was enthusiastically received at Covent Garden on his first appearance, and still more so when at the same theatre he took his place as conductor of Oratorio concerts.

On the 9th of March the rehearsals of 'Oberon' began, and on the 9th April he completed his overture, writing at the end of his score, as was his wont, "*Soli Deo Gloria!*" There is no similar instance in the history of music of a work at once so tender and so vigorous, so original and yet so musician-like, being written under such conditions. It also embodies more than any other the bold innovations he introduced in the construction of his last overtures. The generally adopted plan of the most celebrated composers, with regard to the instrumental introductions of their grandest works for the stage, had always been to "prepare" the minds of the auditors rather than "anticipate" effects which belonged to the "action" of the musical drama, and arose from situations or incidents in the course of its development. With Gluck, Mozart, Cherubini, and even Beethoven, an overture was a complete piece of music by "itself," quite independent, and nearly in every instance without any "direct" reference to the lyrical drama it preceded.

In his earlier works, 'Sylvana,' 'Peter Schmoll,' 'Turandot,' 'The Ruler of the Spirits,' &c., Weber had followed in the footsteps of his predecessors, but with the 'Freyschütz' he began to tread an entirely new path. Employing the chief materials of his opera, moulding and knitting its various elements together, like the laborious and skilful workmen of Florence or Rome, the joints and rivets of this artful mosaic became only discernible to the practised ear of the musician. His idea was to give a complete epitome—nay, the very essence of the opera to follow—*before* the rise of the curtain. This system, according to the most competent critics, interfered materially with the perfect musical form of his overtures; but the effect produced was magical; and though perhaps deficient or faulty in construction, their success is even more universal now than at their first appearance, nearly fifty years ago. There can be no doubt that for colouring, and poetical characteristic feeling, these orchestral preludes are unparalleled and matchless.

At last the 12th April arrived. The house was crammed to suffocation—Weber's reception perfectly overwhelming. The overture was encored. Huon's great air, sung admirably by Braham, was re-demanded, as were the ballad and romance of 'Fatima,' sung by Madame Vestris. When the curtain fell there occurred an incident as yet unprecedented in England. The name of Weber was called louder and louder still, until the whole house joined in one cry, and the composer, trembling and exhausted, appeared before the applauding crowd, which now burst into a perfect frenzy of enthusiasm. His task being accomplished, his almost indescribable yearning for home and a bright sky increased with every day. Each stipulated representation of his 'Oberon' under his own direction was scored off with trembling hands. It was with a heavy heart that the suffering man now proceeded to play his last trump-card, upon which his greatest hopes were fixed. He began the preparations for his concert. The celebrated Miss Stephens (present Dowager Countess of Essex), of whose talent Weber was a great admirer, had offered to appear at his concert.

Being desirous of singing some new composition by the Master, Weber could not resist her wish. The words chosen were from Moore's 'Lalla Rookh'—"From Chindara's warbling fount I come"; but the effort for him was fearfully painful, and twice he flung down his pen in utter despair. At last, on the morning of the 18th May, the great artist's fleeting genius came back once more, but the poor hands were unable to write down more than the melody for the voice. From this sketch Weber rehearsed the song with the celebrated artist, and accompanied it from memory at the concert. Unfortunately the evil star, of which he so often complained, had not forsaken him on this occasion. On the 26th May, the day of the concert, Epsom Races took place. The same afternoon Signor Begrez gave his concert at the Duke of St. Albans', at which all the high society of London was present. Torrents of rain poured down from heaven, and the concert hall in the Argyle Rooms was almost empty. At last

Weber tottered in supported by Sir George Smart. He looked around, a bitter, painful smile distorted those features, usually so placid in the midst of every suffering. Miss Stephens warbled his song to the words of Moore. These tones were the last he ever produced. The enthusiastic applause of the scanty audience failed to cheer the heart of the dying man. After the closing note of the overture to 'Euryanthe,' Fürstenau led him from the room. He fell on a sofa in an utterly shattered state, almost breathless. Friends and artists surrounded him, but he gave little heed. He only murmured in a faint voice, "What do you say to that? That is Weber in London."

His heart was broken. On the first of June the painful symptoms of the poor sufferer had so much increased that Dr. Kind considered his state highly precarious. His friends wrung from him the promise that he would conduct no more, and even give up his own benefit. This resolution appeared to bestow fresh spirits on him. It enabled him to hasten his return. One thought alone occupied his whole soul, to be at home again amongst his own, to see them if but once. He fixed his departure for the 6th, arranged all his pecuniary affairs with minuteness, and employed his friends in purchasing presents for his family in Dresden. The attempt appeared impossible to all, but not to increase his irritation they seemed to enter into his views. In his last letters to his beloved Lina, he pours out his loving soul. He writes thus with a gleam of his old spirit: "You will *not* have many more letters from me, and so receive now my high and mighty commands. Do not answer this to London, but *poste restante, Francfort.* You are astounded; well, I am not coming home through Paris; what should I do there? I cannot walk, I cannot speak. I will have nothing more to do with business for years to come; so it is far better I should take the straight way home by Calais, through Brussels, Cologne, and then by the Rhine to Francfort. What a charming journey! I must travel very slowly, however, and probably rest for half a day now and then. I shall gain a good fortnight thus, and by the end of June I hope to be in your arms."

Once more, on the 2nd of June, his trembling hand traced the following lines:—"What a joy, my own dear darling, your letter gave me. What a happiness to me to know that you are well. As this letter requires no answer, it will be but a short one. What a comfort it is not to have to answer. Oh! were I but amongst you again. I kiss you with all my heart and soul, my dearest one. Preserve your affection for me, and think of him who loves you above all." (Thy Carl.) On the evening of the 4th another friend of Weber's, Göschen, the father of the present Secretary of State, came with Moscheles to take leave of him. He spoke almost in a whisper, but always of his journey. At ten o'clock the friends Smart and Fürstenau insisted on his retiring, and conducted him to his room. He refused the latter's offer to sit up with him, and would not even leave his door unlocked. His last words were, "Now let me sleep." The next morning Sir George Smart's servant knocked early at the door, as he was wont to do; not receiving any answer he knocked again, and louder. The alarmed servant rushed to Sir George, who hurried to the room. Fürstenau was sent for; to their repeated knocking no answer was returned. It was now resolved to force the door. All was still within. Only the ticking of the watch was heard. The bed-curtains were torn back. There lay the beloved friend, his head resting on his left hand, as if in tranquil sleep—not the slightest trace of pain and suffering on his noble features. The soul yearning for the dear objects of its love had burst its earthly covering and fled. There was peace after all the struggle. The immortal master was not dead; he had gone home. Early in the morning of the 21st of June, a long line of mourning carriages wound their slow and weary way to the far distant chapel of Moorfields. To the solemn strains of the 'Dead March in Saul,' and Mozart's 'Requiem,' sung by Miss Stephens, Miss Paton, Braham, and Lablache, the coffin had been lowered into the vault, and there, far from home and all his heart prized most, rested the body of Carl Maria von Weber.

Years passed away, but it seemed as if that yearning for home which was the great master's last feeling, had not died out with him. It continued to haunt those he had so fondly loved. In the year 1844, mainly by the energy of Richard Wagner, then Capellmeister at Dresden, and a stanch friend of the widow and children, Weber's last wish had been fulfilled. He was again to be at home, and the flowers on his grave were tended by his beloved Lina. Richard Wagner's farewell to Weber at his grave, though in rather high-flown language, may be quoted here, as embodying the feelings of his countrymen:—"There never was a more German composer than thou. In whatever fathomless realms thy genius carried thee, thou didst remain chained by the most tender links to the heart of thy German people, with whom thou didst cry or smile like a believing child listening to the legends and tales of its country. Yes, it was thy child-like simplicity which guided thy manly spirit like a guardian angel, and kept it pure and chaste, and that purity was thy chief characteristic. Till death didst thou preserve that supreme virtue. Thou wouldst never sacrifice or alienate this beautiful inheritance of thy German origin; thou couldst never betray us. Behold, the Briton does thee justice, the Frenchman admires thee, but only the German can love thee! Thou art his own, a bright day in his life, a drop of his blood, a particle of his heart!"

[J. B.]

## GENERAL MONTHLY MEETING,

Monday, February 2, 1874.

THE DUKE OF NORTHUMBERLAND, D.C.L. President,
in the Chair.

The Rev. Frederic Braithwaite, M.A.
Robert Stephen Faulconer, Esq.
The Right Hon. G. J. Goschen, M.P.
The Lady Claud Hamilton,
Henry Forsyth Harwood, Esq.
Surtees Harwood Harwood, Esq.
Robert John Taylor, Esq.
Joseph Cæsar Zambra, Esq.

were *elected* Members of the Royal Institution.

With reference to the Letter from the Managers and Members to the Family of their late President, the following Letter from his son, SIR HENRY T. HOLLAND, Bart. to the Secretary, was read:—

"65, RUTLAND GATE,
"*December* 5, 1873.

"MY DEAR SPOTTISWOODE,

"I have just returned from abroad, and found your letter enclosing the letter from the Managers and Members of the Royal Institution.

"I beg you to return to them the sincere thanks of myself and of the other members of the family for the kind expressions of sympathy contained in their letter.

"My father received many honours in his lifetime, but I think none gave him greater satisfaction, or was more highly valued by him, than his election to be President of the Royal Institution. It is, therefore, deeply gratifying to us to learn how truly his conduct in that high office was appreciated by those who had the best opportunity of estimating the way in which he performed his duties as President.

"May I beg you to bring my reply under the notice of the Managers, and to assure them that their letter will be treasured by us as a most valuable record of our father's services, and still more of the affection and regard entertained towards him by those for whom and with whom he worked for so many years.

"Believe me to be, very truly yours,
"H. T. HOLLAND."

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

*Actuaries, Institute of*—Journal, No. 93. 8vo. 1873.

*Anonymous*—The Empire of Brazil at the Vienna Universal Exhibition. 8vo. 1873. (With Maps.)

*British Architects, Royal Institute of*—Sessional Papers, 1873–4, Nos. 2, 3, 4. 4to.
*British Museum Trustees*—Handbook of Edentate, Thick-skinned, and Ruminant Mammals in the British Museum. By Dr. J. E. Gray. 8vo. 1873.
Catalogue of Prints and Drawings. Division I. Political and Personal Satires, Vol. II. 1689–1733. 8vo. 1873.—Facsimiles of Ancient Charters. Folio. 1873.
*Civil Engineers, Institution of*—Minutes of Proceedings, Vols. XXXV. XXXVI. 8vo. 1873.
*Clarke, Hyde, Esq. (the Author)*—Memoir on the Comparative Grammar of Egyptian, Coptic, and Ude. (K. 99) 8vo. 1873.
*Editors*—American Journal of Science for Dec. 1873. 8vo.
Astronomical Register for Dec. 1873 and Jan. 1874. 8vo.
Athenæum for Dec. 1873 and Jan. 1874. 4to.
Chemical News for Dec. 1873 and Jan. 1874. 4to.
Engineer for Dec. 1873 and Jan. 1874. Folio.
Food Journal for Dec. 1873 and Jan. 1874. 8vo.
Journal for Applied Science for Dec. 1873 and Jan. 1874. Folio.
Journal of Gas-Lighting for Dec. 1873 and Jan. 1874. 4to.
Nature for Dec. 1873 and Jan. 1874. 4to.
Nautical Magazine for Dec. 1873 and Jan. 1874. 8vo.
Once a Week for Dec. 1873 and Jan. 1874. 8vo.
Pharmaceutical Journal for Dec. 1873 and Jan. 1874. 8vo.
Quarterly Journal of Science for Jan. 1874. 8vo.
Telegraph Journal for Dec. 1873 and Jan. 1874. 4to.
*Elliot, The Lady*—The History of India as told by its own Historians, by Sir H. M. Elliot, Vol. V. 8vo. 1873.
*Greenwich, Royal Observatory*—Greenwich Observations for 1871. 4to. 1873.
Description of the Water Telescope (in Observations, 1871). 4to. 1871.
*Hall and Co. Messrs.*—The Treasury of Languages. 16to. 1874.
*India Office*—Meteorological Observations at Simla, 1841–5. 4to. 1872.
*Jablonowski'schen Gesellschaft*—Preisschriften XIV. XV. 8vo. 1869.
*James, Sir Henry, R.E. F.R.S. &c.*—Comparison of Standards of Length of England, France, &c. By Capt. A. Clarke. 4to. 1866–73.
*Lisbon Royal Academy of Sciences*—Memorias da Academia, Nova Serie.
I^a^ Classe: Tomo I.–IV. 4to. 1854–70.
II^a^ Classe: Tomo I. II. Parte 2. III. IV. Parte I. 4to. 1854–72.
Jornal de Sciencias Mathematicas, Physicas, e Naturaes. Tomo I.–III. 8vo. 1868–71.
Portugalliæ Monumenta Historica:—
Leges et Consuetudines. Tomo I. Fasc. 1–6. Folio. 1856–68.
Scriptores. Tomo I. Fasc. 1, 2, 3. Folio. 1856–61.
Diplomata et Chartæ. Tomo I. Fasc. 1, 2, 3. Folio. 1868–72.
*Liverpool Literary and Philosophical Society*—Proceedings, No. 27. 8vo. 1873.
*Lubbock, Sir John, Bart. M.P. F.R.S. M.R.I. (the Author)*—Origin and Metamorphoses of Insects. 16to. 1873.
*Manchester Philosophical Society*—Proceedings, Vol. XIII. Nos. 5, 6. 8vo. 1873.
*Mechanical Engineers' Institution*—Proceedings, July 1873, Part 1. 8vo.
*Pharmaceutical Society*—Catalogue of Library. 8vo. 1873.
*Photographic Society*—Journal, Nos. 245, 246. 8vo. 1873.
*Royal Society of Literature*—Transactions. 2nd Series, Vol. X. Part 3. 8vo. 1873.
*Swanwick, Miss Anna, M.R.I. (the Translator)*—The Dramas of Æschylus: with thirty-three Illustrations from Flaxman's Designs. Folio. 1873.
*Upsal Royal Society of Sciences*—Nova Acta, Serie III. Vol. VIII. Fasc. 2. 4to. 1873.
Bulletin Météorologique Mensuel de l'Observatoire de l'Université d'Upsal. Vol. IV. Nos. 1–12. Vol. V. Nos. 1–6. 4to. 1872–3.
*Yorkshire Archæological Association*—Journal, Parts 9, 10. 8vo. 1873.
*Zoological Society of London*—Transactions, Vol. VIII. Part 6. 4to. 1873.
Proceedings, 1873. Parts 1, 2. 8vo.

## WEEKLY EVENING MEETING,

Friday, February 6, 1874.

Warren De la Rue, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

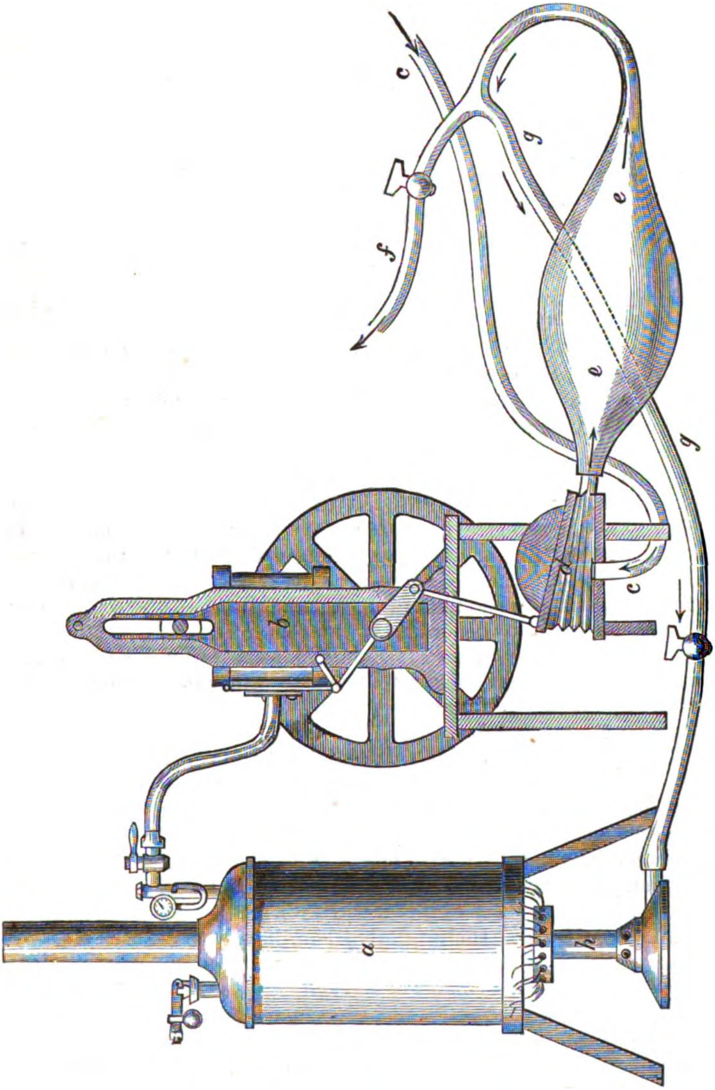
Alfred H. Garrod, Esq.
Fellow of St. John's College, Cambridge,

### *On the Heart and the Sphygmograph.*

In the same way that by the spectroscope much can be learned as to the chemical constitution and the physical changes going on in the sun, so by the sphygmograph applied to the artery at the wrist many of the most important phenomena occurring in the heart can be studied with a facility that cannot be otherwise attained. Till the introduction of the sphygmograph of Marey the pulse was considered to be little more than a simple up and down movement, because the instruments employed to register it, such as those of Herisson, Ludwig, and Vierordt, developed so much momentum that the details of the true trace were disguised. In the instrument as at present employed, the substitution of counterbalancing springs instead of weights has so far improved its efficiency, that the pulse is now known to form a decidedly complicated curve if its movements are allowed to record themselves on a moving paper. The sphygmograph trace, as thus produced, gives indications in two directions; first, as to the action of the valves of the heart; and secondly, as to the manner in which the muscular walls of the ventricles perform their work. It is to the former of these subjects that most physiologists have directed their observations in employing the instrument; but it is to the latter, the more important of the two, that it is my intention to direct attention on the present occasion.

The heart, being nothing more than a pump of a peculiar construction, much may be learned by comparing it with other artificially constructed machines for the same purpose. In most such machines the force which keeps the pump at work is constant in power: in other words it does not vary automatically in efficiency with the amount of work that is expected of it. In the locomotive engine, however, there is an arrangement by which the furnace becomes hotter as the speed at which it moves is increased, the waste steam pipe opening into the funnel and so varying the amount of the draught through the boiler tubes. With this arrangement it is nevertheless evident that there is a great waste of fuel in the construction of the furnace.

It is quite possible to construct a steam-engine on much more



Automatically-working Engine, when supplied with coal-gas.

economical principles, and the accompanying figure illustrates the manner in which the small engine on the table is at present working (see Figure). The boiler (*a*) being sufficiently heated, drives the engine (*b*), which performs work by pumping *coal-gas* from the tube *c* through the pump *d*, into the elastic reservoir *e*. From this elastic bag most of the coal-gas escapes, through the tube *f*, into an ordinary gas-bag, but a tube (*g*) carries some of it to supply the Bunsen's burner (*h*) which heats the boiler. It is evident that with this arrangement the size of the flame of the Bunsen's burner (*h*), and therefore the pressure of steam in the boiler, which is the same as saying the efficiency of the engine, varies with the amount of work required of that engine; for the greater the pressure in the elastic bag, the harder is it for the engine to perform the work required of it, and the greater is the burner-flame. With a certain proportion between the sizes of the orifices of the taps and the extensibility of the elastic bag and tubes, it would be possible to arrange this engine in such a manner that, within certain limits, the velocity of the fly-wheel would not vary with the pressure in the elastic bag; in other words, with the work to be done. That the heart is a pump, constructed on the same principle as this engine, is the teaching of the sphygmograph, as far as it is in my power to interpret its curves; the proof resting on the following considerations.

First, the analogy between the anatomical distribution of the arteries and the different parts in connection with the engine is not difficult to trace. The coal-gas corresponds to the blood, the boiler (*a*) together with the engine (*b*) to the muscular tissue of the heart, whose left ventricular cavity has its analogue in that of the bellows (*d*). The elastic reservoir, together with the tubes, corresponds to the systemic arteries, the gas-bag connected with the tube *f* (the capillaries) to the systemic veins; and the tube *g* to the coronary arteries, which supply the muscular tissue of the heart with nutrient blood, just as it does the boiler by means of the burner *h*. This, however, does not show that the pumping power of the heart varies directly as the blood-pressure; that such is the case depends on the opportunity offered by the sphygmograph trace for the estimation of the length of the ventricular systole under different circumstances. Each beat or revolution of the heart is divided into two main parts—(1) the period of contraction or systole, and (2) the period of repose or diastole. The former of these occupies the interval between the commencement of the primary rise and that of the dicrotic rise in the sphygmograph trace; the latter from the dicrotic rise to the commencement of the succeeding primary rise. In all good tracings from healthy pulses, these two points, the primary and dicrotic rises, are readily found; and their relative lengths can be estimated with great accuracy. A large number of measurements have enabled me to prove that *the relative lengths of the systolic and diastolic portions of the pulse-trace do not vary for any given pulse-rate.* But, as will be granted by most physiologists who have worked at the subject, *the blood-pressure in the arteries is quite independent of the pulse-rate.* Consequently the heart may be doing very different amounts of work without any variation in the pulse-rate, which is the same thing as saying with the same length of systole; which makes it evident that, as in the above-described engine, *the force of the cardaic muscular contraction varies directly as the blood-pressure*, knowing what we do about the flow of fluids through capillary tubes, and the capacity of the arterial system under different degrees of blood-pressure.

The sphygmograph trace tells us more than this. Though the length of the systolic portion of the beat does not change with any given pulse-rate, nevertheless it does so greatly with different rapidities of pulse; my observations showing that the length of the systole varies as the cube root of the whole beat, being found from the equation $xy' = 47 \sqrt[3]{x}$ when $x$ = the pulse-rate and $y'$ = the ratio borne by the systole to the whole beat. From this no other inference can be drawn than that the length of the diastole, or period of cardaic rest, during which fresh blood is circulating through the ventricular walls, must modify the contractile force of its muscular substance. The exact extent of this influence can be more readily estimated by a study of the cardiograph trace, which is obtained by applying the sphygmograph to the side of the chest at any spot where the pulsations of the heart are to be felt. It may, from the thus obtained curves, be demonstrated that, if not exactly, approximately at least, *the nutrition of the heart's walls must vary as the square root of the length of the diastolic period.*[*]

There is much, therefore, as I hope I have been able to show, to be learned respecting the action of the heart from measurement of sphygmograph tracings, and it is scarcely too bold to extend the generalization to the properties of muscular tissue generally; for the fact that each beat depends entirely for its efficiency on the peculiarities in the blood-pressure and the duration of the previous diastole, removes all complication as to incompleteness of exhaustion, and all doubts as to the exact amount of work done by the muscular fibres themselves of that most perfect of engines, whose extreme perfection enables it to complete in most of us something like 750,000 beats in a week, and nearly thirty thousand million revolutions in a person by the time he is seventy years of age.

[A. H. G.]

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[*] 'Journal of Anatomy and Physiology,' May and November, 1873.

## WEEKLY EVENING MEETING,

### Friday, February 13, 1874.

William Spottiswoode, Esq. LL.D. Treasurer R.S. Secretary and Vice-President, in the Chair.

### John Doran, Esq. Ph.D. F.S.A.

## *On the Opponents of Shakespeare.*

Dr. Doran, after commenting on the little knowledge we possess of the life of Shakespeare, and the absence of any of his autograph letters, characterized Dryden as his first and greatest opponent, than whom no one had more disparaged that poet, although no one had spoken of him in higher terms of praise. The Monarchy and the stage fell together; but Shakespeare was re-enthroned at the Restoration, soon after which Dryden began his jealous attacks by mutilating and horribly debasing 'The Tempest.' To the indignation of the Shakespearian public Dryden replied by some scornful lines in the Epilogue to 'The Conquest of Granada' (Part 2), and then attempted to justify himself in his vigorous pamphlet, 'The Defence of the Epilogue,' in which he speaks of Shakespeare as uncultivated by education or by converse with courts. Dryden placed Shakespeare on a level with Fletcher. He maintained that there was not a page in either poet that was not disfigured by "some solecism in speech, or some notorious flaw in sense," and he subsequently adds, "of language." In this 'Defence' Dryden pronounced the 'Winter's Tale' 'Measure for Measure,' and 'Love's Labour Lost,' to be so meanly written that the comedy neither caused your mirth nor the serious part your concernment. Dryden did not hesitate to say that Shakespeare was open to as much contempt as admiration; that he was as often asleep as awake: and that he constantly fell into "a carelessness and a lethargy of thought." Dryden asserted that in the hands of poets since Shakespeare, the English language had improved; he ascribed to Waller the merit of introducing the "well placing of words for the sweetness of pronunciation!" and he found a proof of the weakness of Shakespeare in his (alleged) inability to carry out the character of *Mercutio* beyond the third act. Dr. Doran proceeded to show the ill effects of Dryden's general course, 1st, on dramatic criticism, as illustrated in the persons of Rymer and others who followed Dryden's lead. 2ndly, on the stage, where Dryden was the real founder of that abominable school of English comedy, the appalling indecency of which was not compensated for by any wit; and 3rdly, on Shakespearian literature, that is, on Shakespeare's plays, which were treated as Dryden had treated 'The Tempest,' by a long line of the opponents of Shakespeare, who called themselves his "improvers." The speaker showed how these improvers had missed both the object of the poet and the moral of his plays, by examples from Davenant's perversion of 'Macbeth' to Lord Lansdowne's as absurd perversion of 'The Merchant of Venice.' Dr. Doran described Jeremy Collier's denunciation of the theatre as Dryden and his followers had made it, to be in reality an advocacy of "the well-trod stage" of which Milton had approved; and he rendered such justice as was due to Dryden, who in the noble 'Ode on the Death of Mrs. Killigrew,' expressed his grief at having flooded the English stage with pollution, but who got the better of his grief, wrote comedies in the old immoral strain, and laid the blame on the town, which, he said, liked nothing better.

Dr. Doran next referred to "the Shakespeare Ladies," and "the Beaumont and Fletcher Ladies," who are noticed in Fielding's play, 'The Historical Register.' He explained the objects of these respective opponents and supporters of Shakespeare, and narrated how the Shakespeare ladies enabled the managers to put his plays on the stage, by liberally subscribing towards their being splendidly represented and according to the original text. This was described as a fitting preparation for the coming of one of the most intelligent interpreters of Shakespeare's sentiments, and one of the noblest representatives of Shakespeare's heroes—David Garrick. When Garrick visited Paris, he met the fiercest of Shakespeare's opponents on the Continent, in the opinions of Voltaire. Dr. Doran illustrated the history of this opposition by various anecdotes; he showed also how Ducis attempted to adapt Shakespeare's plays to Voltaire's so-called "system," and how the adapter became more Shakespearian at each attempt. Having sketched the history of the various endeavours to place Shakespeare's plays on the French stage, and having told how the English poet triumphed there through the great actor, Talma, and how he has been added to the stock of French poetry by the admirable translation of François Hugo, Dr. Doran concluded by pointing out the rarity of the true poet, and insisting that no man could be *supreme* in the *sublimest* faculty of poetry, but under a divine influence. This influence enabled Shakespeare to display the mightiest of poetical power through the very simplest of means,—enthralling the sense by, perhaps, a few one-syllabled words; and Dr. Doran asserted that in the *best* teachings of the *best* of his poetry, Shakespeare was not only our national poet, but more essentially, practically, and profitably, our national saint than any one of the Worthies who are called the Champions of Christendom. "Their shrines," said Dr. Doran, "are but the impalpable webs of Fancy; their works only the fond imaginings of Romance; but the shrine of our Shakespeare still sanctifies his native Avon, and his works are, for the civilized world, a joyous possession for ever."

## WEEKLY EVENING MEETING,

Friday, February 20, 1874.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Vernon Heath, Esq.

### *On the Autotype and other Photographic Processes and Discoveries.*

Photography, young as it is amongst the sciences, and still younger in its alliance with art, has made during the comparatively few years of its existence the most astonishing progress.

In the year 1842 I was working out the marvels of Mr. Fox Talbot's calotype process with something more than merely keen interest, for his discovery was to me an absolute realization of that which for a long time had been continually the subject of my thoughts. I was accustomed, in the days I refer to, to sketch a great deal out of doors, using for assistance a form of camera-obscura which was then made for that purpose, and, with the image before me, which the lens of my camera formed on its copying screen, how often did I wonder whether, within the wide realms of chemical science, there existed means by which that image could be secured! Day after day I thought and dwelt upon this, as others no doubt had done before, until at last there came one eventful Friday evening. On the 25th January, 1839, I had the privilege to be present in this theatre, and hear, to my intense surprise and gratification, Mr. Faraday announce the two discoveries—the daguerreotype and Mr. Fox Talbot's invention, then named by him "photogenic drawing." Mr. Faraday invited his audience to inspect the specimens displayed in the library of this Institution, and I, being one of those who did so, was from that moment in heart and spirit a disciple of the new science—Photography.

Here are the results from some negatives I obtained last summer; but let me for a moment go back to the period I just now referred to and its results. Here are prints from some calotype negatives I obtained during the summer of 1842. Let me show you one of these prints by the side of this (the calotype print was 6 × 4½ inches, the enlarged autotype 48 × 38 inches), in order that, in some measure, you may see the degree of progress that has been made in so short a period as thirty-one years. I am sure that I may be suffered to say that to myself this is a comparison of extremely great interest, though I doubt whether I should have drawn your attention to it but that it enables me, by pointing to the progress it evinces, to say that the advance photography has made in all its branches and applications is just as marked and equally as great. But, successful as it has been, varied as are its uses, it has had to contend with one very serious drawback—the unstable condition of the photograph itself. All photographs by any of the ordinary methods are subject to gradual and spontaneous chemical change, from which arises their want of permanence. This, though, is so well and so generally known that it is unnecessary that I should dwell upon it, beyond saying that as soon as it was recognized it became the end and aim of a large number of patient, earnest workers, in all parts of the world where photography was known, to provide a remedy; and that now, to quote from an article in 'The Times' upon this subject, "by the cumulative effect of successive inventions and improvements, means have been found for accomplishing this end."

Of the processes which have had this object in view the autotype is entitled to take very high rank, both for the beauty and completeness of its results. The word "autotype" signifies, as you will have determined, self-printing; and by those who proposed and adopted it it was intended to express the power of producing, independently of any aid or action other than that which belongs to the process, a picture in monochrome, "whether," to use the language of a well-known writer, Mr. Tom Taylor, "it be in red or black chalk, India ink, sepia, common ink; in short, any colouring matter that can be incorporated with gelatine;" and he adds that "as only the pigments which are the most permanent that are known to art are used in the process the finished autotype is as permanent as the pigment which is selected for its production."

In the explanation I am about to give you of autotype printing and productions, I will ask permission to pass over a great deal of that which is historical, as well as a great deal of that which is technical. I fear that this is but scant justice to the process, as well as to its numerous discoverers; but even if time permitted me, I doubt whether I should be able to do the justice I desire to the many eminent persons who have been associated, in some form or other, with its discovery or applications.

I must content myself with giving an explanation of its most important principles, and with mentioning merely the names of those whose researches and discoveries have, during the last few years, brought it to its present state of perfection and ease of manipulation. Foremost among these are Mr. Mungo Ponton and M. Poitevin; the late Mr. Blair, of Perth; prominently, Mr. Swan, of Newcastle-on-Tyne; materially and importantly, Mr. Johnson, of the Autotype Fine-Art Company; then Mr. Ernest Edwards and Mr. Window; and last, though very far from least, Mr. J. A. Spencer and Mr. Sawyer who are now members of a well-known firm engaged in the production of autotype work. For assistance and contributions for this evening I am greatly indebted to several persons; but to Messrs. Spencer, Sawyer, Bird and Co., I am under the greatest obligations, for they have not only supplied me from their factory at Ealing Dean with means and appliances for my discourse, but Mr. Spencer has very kindly given me the aid of his valuable services for my demonstrations.

Gelatine plays a most important part in the autotype and all its kindred processes. The colour of an autotype picture is due alone to the pigment which is used. Here is some gelatine in the state it is before the pigment has been incorporated with it; and here some with which various colours have been mixed—"pigmented gelatine," as it is called. Paper suitable for the purpose is coated on one side with this preparation, and if not needed for immediate use is allowed to dry. Here are a number of sheets which have been so treated. In this state it will keep any reasonable time; light has no action upon it; it may even, without injury, be soaked in cold water and again dried; but if put into warm water the gelatine is dissolved, and the pigment washed away.

To render this preparation photographic—that is, to render it sensitive to the action of light—it has to be immersed for a short time in a solution of bichromate of potass. Here is a sheet which has been so immersed, though in no way that is apparent are certain properties of the original preparation altered; that is, it will keep a reasonable time if not exposed to light, and it is still readily soluble in warm water.

But expose this film of bichromatized gelatine to light and a most important action is set up. Ordinary photography, you know, is based entirely on the property light possesses of acting on nitrate of silver in contact with organic matter, the colour of an ordinary photograph being due to the reduction of the silver, and its subsequent toning with gold. But in the autotype, and all like processes, the photographic action is not so much a colour change as in the silver print, but a change in the character of the bichromatized gelatine of this nature—that which, before exposure to light, was readily soluble, becomes by exposure to light, insoluble. By the way, it will be convenient now and hereafter to call this compound of gelatine and colour by the name that has been given to it—"tissue." It is unsensitized tissue before immersion in the bichromate, sensitized tissue afterwards.

Suppose, now, we place this card, with the openings I have cut upon it, on a piece of sensitized tissue, and expose them to light in the ordinary printing-frame of the photographer, all those portions of the tissue which can be seen through the openings of the card, are, by the action of light, rendered insoluble, while the protected parts remain soluble and can be readily washed away, giving in the end the result I have here. Now, although this illustration answers my immediate purpose, and exhibits the particular change I have referred to, it explains little indeed of the whole change light is able to effect in the sensitized tissue. This, as we go on, we shall see is far more interesting, far more important, and, I may add, far more wonderful. The absolute action of light upon the tissue may be epitomized thus:—The degree of insolubility that is attained, or the degree of solubility that is preserved, is in exact proportion to the degree in which light is shut out from, or let into, the coating of bichromatized gelatine; that is, suppose we intervene between the tissue and the light a means of resistance constructed on an exact scale of gradation of tones running from transparency to opacity, we should translate that resistance into the same exact gradations of the pigment which had been mixed with the gelatine.

[This proposition, involving as it does the most important principles of the process, was fully explained and illustrated by the use of a large diagram, by which the relative degrees of insolubility and solubility were exhibited as the result of the light's action under certain particular circumstances; the diagram being also used for the purpose of showing that where the insoluble film was the thickest there would be the darkest tone of colour, and where it was the thinnest there was the lightest tone, and that between those extremes all the other gradations of tone are formed. A large autotype landscape of the *Vale of Festiniog*, from a negative taken by the speaker, was referred to as proof of this, the varying planes of distances being rendered in the most perfect way. Mr. Spencer and his assistants then proceeded to develop an autotype print of unusual size, the speaker again using the diagram for the purpose of explaining why the transfer paper was attached and the original paper support removed. Mr. Spencer succeeded most admirably in the development of the print, the subject of which was *Windsor Castle from the Thames.*]

The speaker, having explained certain points which it was necessary to make clear during the process of developing his Windsor Castle picture, said:—Now, regarding autotype pictures of this character, and of the larger size behind me, since this discourse was announced I have been so frequently asked to explain the special method employed for their production that I am led to think it has sufficient general interest to justify my doing so, especially as my description will be very short. Our illustration here, as well as these and the many other examples which have at different times been exhibited in the library of this Institution, are all enlarged from negatives of this size (12 × 10 inches). The method used is extremely simple, and is itself of much earlier date than the autotype process. I find from the 'Journal of the Photographic Society of London,' that nearly twelve years ago I described, at one of the meetings of the Society, a plan of enlarging which at that time I was using extensively, and which, practically, is identical with that now used for the production of these pictures, with the exceptions that an *autotype transparency* is substituted for a silver-on-collodion one and an *autotype* for the final resulting print in lieu of the usual silver print. The operations are these:—A tissue print is made from and by contact with the negative to be enlarged, and this print, being transferred for its permanent support to clear transparent glass, is therefore what is termed a "transparency." This tissue transparency, if made from a good negative, is extremely beautiful, and is immeasurably superior to any photographic print that could be obtained from the same negative by ordinary means, and greatly superior to a transparency produced by the old method of silver-on-collodion. It is really to the perfect character and quality of the autotype transparency that the great success of the enlargements resulting from it is due. The merit of first using a tissue transparency for enlarging is, I believe, due to Mr. Window, of Baker Street.

Suppose the transparency is in all respects satisfactory, it is set up in front of a lens; the size of the resulting negative depending upon the focal length of the lens, and on the distance the transparency, the lens, and the position for the negative are apart. The negative so produced is an ordinary silver-on-collodion one, and to avoid inversion in the resulting autotype print the transparency is put film side outwards. As to magnitude, a negative can be produced of any size within the limits of a camera and its adjuncts. The size of the plates for the negatives from which these pictures are produced are each 48 × 38, and I see no difficulty in obtaining equally perfect results on plates double the size. By the exercise of care and skill in the manipulation there need be no loss of detail or sharpness; whatever in such respects is the condition of the original negative, so is the condition of the reproduction, without reference to the amount of enlargement.

Doubtlessly a great deal is due to the ability and technical knowledge Messrs. Spencer, Sawyer, Bird and Co. have brought to bear in working out the details of the process; but the character and quality of the original negative affects most materially the character and quality of the resulting enlargement. Partly due to the long period over which my own experiments in enlarging processes extend, my landscape negatives are found to possess a distinctive and peculiar fitness for autotype work; but, as this is a matter I could not, nor should not, discuss, I will merely say that, in the event of anyone desiring to see the character of my negatives, I have had four representative specimens put in the library.

I pass now to the concluding branch of my discourse. Thus far my explanations have been limited to considering the autotype process as a substitute for certain of the usual photographic processes; but it has yet another application, the nature of which is such that those who hear of and see it for the first time do so with very great surprise. Poitevin, who was one of the early discoverers of that which resulted in autotype, found that the surface of gelatine rendered insoluble by light acquired the property of repelling water as from a greasy surface, thus enabling a design to be inked with printing-ink and an impression to be taken from the inked surface on to paper—a process analogous to the operation known as lithography. Founded on Poitevin's discovery is that which is now known as the "mechanical autotype printing process," by which, on a plate prepared more or less by the means used for autotype productions as I have explained them, a print from a negative is obtained, and the plate being then placed in a printing-press copies of it can be produced in printers' ink or in any permanent pigment with great rapidity; whilst the proofs themselves, in their delicacy of texture, gradation, and faithful rendering of the minutest detail, leave little to be desired.

The principle upon which this method of printing is based is this:—Avoiding the actual details of the manipulation, a plate, which in this case is a thick piece of finely-ground glass, is coated with a solution of gelatine and bichromate, and, when dry and hard, is exposed under a negative. Much—indeed all—depends upon the proper hardening of the film, the method of accomplishing this being that for which a patent was taken out in October, 1869, now the property of the Autotype Fine-Art Company.

After the exposure under a negative it is washed in *cold*, not hot, water, as in the ordinary autotype process—not with a view to remove any portion of the gelatine, but simply to get rid of the bichromate, and to permit the gelatine to become saturated with moisture, which it will absorb in the inverse ratio of the action of light. This film, after treatment with water, is just in the condition of a moistened lithographic stone. If a roller charged with greasy ink be applied to its surface, the ink will be repelled by the moist portions, but will adhere to the dry or insoluble portions, as it would to the greasy image on the stone. The ink adheres to the image in the exact ratio of its freedom from moisture. An exact transcript of the negative is thus secured in printing-ink on the layer of gelatine, and a sheet of paper being placed upon it, and suitable pressure applied, a print is produced.

By the kindness of Messrs. Spencer, Sawyer, Bird and Co. I am enabled to show you the production of prints by this method. Here, in its place in this printing-press, is a plate upon the surface of which is a result obtained from contact with a negative and exposure to light as in our former illustrations. And now, as Mr. Spencer has been good enough to bring with him one of his skilled assistants, I cannot do better than leave the completion of the demonstration in his hands. The operations are, as you see, strictly analogous to those of lithography. The ink adheres to those portions which refused to take the water, and in exactly the same degree as the repellent action was manifested. Now it is ready for printing; so the paper is carefully placed upon it, and now, proceeding just as does the lithographer, we shall then have our result.

This, then, is the autotype process divided into its two classes—that which substitutes the ordinary photographic methods, and that which results from the printing-press in a manner analogous to lithography. To summarize: each class possesses great pictorial excellence and beauty, both render, as you have seen, with the utmost delicacy every gradation of tone; and any tint of monochrome possible to the painter can be obtained in their production, and this, too, with a tenure of permanency which may fairly be assumed to be as secure as the Indian-ink drawing, to which experience enables us to assign a stability of at least some centuries. In both classes there is great simplicity and practicability, the object of each operation is perfectly clear and well understood, each step in the process follows the other in regular and unvarying sequence, and all the manipulations are easily performed. There is no complexity requiring unusual intelligence to understand, and there are no uncertain conditions in which success is left to chance or guess-work.

Before concluding, I ought to say that there are several other processes now in extensive use the basis of which is gelatine and bichromate of potass. The Heliotype, the Woodbury system, and the various processes which, under Sir Henry James, are in use in the Ordnance Departments—all more or less depend upon the use of gelatine and bichromate of potass. Remembering the position Mr. Fox Talbot holds as the pioneer of the photographic processes in this country, it is due to him to mention that he was amongst the earliest—if not really the earliest—discoverers of the gelatine and bichromate of potass process.

In the library you will find, thanks to my many contributors, several interesting and striking illustrations of the various processes I have spoken of. You will find also special applications of these processes: of these I will particularly mention the beautiful specimens of the Woodbury process, and the marvellous reproductions of the Old Masters and other subjects of the greatest interest contributed by the Autotype Fine-Art Company.

Here, then, my task finishes—imperfectly and incompletely, I fear. I will, however, permit myself to hope that I may rely, not merely on your generous and liberal interpretation of it, but that I may even consider I have furnished a fairly sufficient answer to the question I hear so often asked—"What is autotype?" and that from what has been said and done to-night, you will be convinced that in autotype we have an additional and important aid to photography, especially as it affects its progress. On this point I think I cannot more appropriately close my discourse than by using the identical words Mr. Faraday spoke from this place on the January Friday evening when he introduced Mr. Fox Talbot's photogenic drawings. Speaking of them, Mr. Faraday said:—"No human hand has hitherto traced such lines as these drawings display; and what man may hereafter do, now that Dame Nature has become his drawing-mistress, it is impossible to predict."

[V. H.]

## WEEKLY EVENING MEETING,

Friday, February 27, 1874.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Francis Galton, Esq. F.R.S. M.R.I.

### *On Men of Science, their Nature and their Nurture.*

The purport of this discourse is to specify the chief qualities by which the English men of science of the present day are characterized, to show the possibility of defining and roughly measuring the amount of any of those qualities, and to conclude by summarizing the opinions of the scientific men on the merits and demerits of their own education, giving an interpretation of what, according to their own showing, they would have preferred. My data are obtained from a large collection of autobiographical notes, most obligingly communicated to me by a large part of the leading members of the scientific world. Applications were addressed to 180 Fellows of the Royal Society, who, in addition to their "F.R.S.," had gained medals or filled posts of recognized scientific position; 115 answers have already been received, of which 80 or 9 are full and minute replies to my long and varied series of questions. But I can deal with only a few deductions from this valuable material, and must refer to a forthcoming work for the rest.

It is of interest to know the ratio which the numbers of the leading scientific men bear to the population of England generally. I obtain it in this way. Although 180 persons only were on my list, I reckon that it would have been possible to have included 300 of the same ages, without descending in the scale of scientific position; also it appears that the ages of half of the number on my list lie between 50 and 65, and that about three-quarters of these may be considered English. I combine these numbers, and compare them with that of the male population of England and Wales, between the same limits of age, and find the required ratio to be about one in 10,000. What then are the conditions of nature, and the various circumstances and conditions of life,—which I include under the general name of nurture,—which have selected that one and left the remainder? Some may feel surprise that so many as 300 persons are to be found in the United Kingdom who deserve the title of scientific men; probably they have been accustomed to concentrate their attention upon a few notabilities, and to ignore their colleagues. It must, however, be recollected that all biographies, even of the greatest men, reveal numerous associates and competitors whose merit and influence were far greater than had been suspected by the outside world. Great discoveries have often been made simultaneously by workers ignorant of each other's labours. This shows that they had derived their inspiration from a common but hidden source, as no mere chance would account for simultaneous discovery. It would appear that few discoveries are wholly due to a single man, but rather that vague and imperfect ideas, which float in conversation and literature, must grow, gather, and develop, until some more perspicuous and prompt mind than the rest, clearly sees them. The first discoverers beat their contemporaries in point of time, and it is therefore due to them, not that science progresses, but that her progress is as rapid as it is. We must neither underrate nor overrate their achievements. I would compare the small band of men who have achieved a conspicuous scientific position, to islands, which are not the detached phenomena they appear, but only the uppermost portions of hills, whose bulk is unseen. To pursue this metaphor; the range of my inquiry dips a few fathoms below the level at which popular reputation begins.

I proceed to speak of the qualities which the returns specify as most conspicuous in scientific men, and I shall endeavour to make them tell their own tale by quoting anonymous extracts from their communications.

The first in order of importance is energy, both of body and of mind. It appears to be possessed in an unusual degree by three-fourths of the men in my list. I should mention that the list contains a very few names of travellers of extraordinary endurance, such as the late Dr. Livingstone; but that I do not speak of these in the following extracts:—1. "Have rowed myself in a skiff 105 miles in 21 hours whilst undergraduate at Cambridge." (This is, I believe, a feat that not one undergraduate in 500 could do.) "Rowed in every race during my stay at the University; rowed two years in the University crews." 2. "Walked many a time 50 miles a day without fatigue, and kept up five miles an hour for three or four hours." 3. "Excelled at school and college in athletic sports, especially in jumping (18 feet). Almost incapable of mental fatigue up to the age of 38. Usually engaged in literary work until long after midnight." 4. "As a boy of 17, I worked for three months all day and all night with not more than four or five hours' sleep. When full of a subject and interested in it, I have written for seven or eight hours without interruption."

Severe scientific work is often done during the night by men engaged all day in anxious business; thus:—"In early life as a boy, I was engaged in business from twelve to fourteen hours a day, yet always found time to study and make my own instruments. Later on, my studies and scientific work were always accomplished after business hours, and it was generally my habit to commence after dinner, and to work at science until 2, 3, or 4 A.M., and to begin business again at 9 A.M. I never thought of rest if I had anything in hand of interest."

I may mention that energy appears to be correlated with smallness of head, a fact which comes out conspicuously here, although the average circumference of head among the scientific men is great. Energy is also, as we have seen, strongly marked among them; but it is much the more strongly marked among those who have small heads. I have ninety-nine returns, many of which I have verified myself, using the hat-maker's whalebone-hoop and measuring inside the hats. It appears that the average circumference of an English gentleman's head is 22¼ to 22½ inches. Now, I have only thirteen cases under 22 inches and eight cases of 24 inches or upwards. The general scientific position of the small-headed and large-headed men seems equally good; but the fact is conspicuous that, out of the thirteen of the former, there are only two or three who have not remarkable energy; and out of eight of the latter there is only one who has. A combination of great energy and great intellectual capacity is the most effective of all conditions; but, like the combination of swiftness and strength in muscular powers, it is very rare.

The excellence of the health of the men in my list is remarkable, considering that the majority are of middle and many of advanced ages. One quarter of the whole have excellent or very good health, a second quarter have good or fair, a third have had good health since they attained manhood, and only one quarter make complaints or reservations. Here are two examples of excellent health:—1. "Only absent from professional duties two days in thirty years; only two headaches in my life." The next is from a correspondent who is between 70 and 80 years of age. 2. "Never ill for more than two or three days except with neuralgia; no surgical operations except inoculation, drawing of one tooth, and cutting of corns." It is positively startling to observe in these returns the strongly hereditary character of good and indifferent constitutions. I have classified the entries, each entry giving the health of the scientific man, of his father and of his mother respectively, and find as follows: —First, a long row of such terms as these: "Excellent; excellent; excellent;" or "Good; good; good;" then comes another row in which some ailment is specified by the scientific man as affecting himself, and as having also affected one or other of his parents. Examples:—1. "Excellent, but hay fever; father, excellent, but severe hay fever." 2. "Good in early life, subject to headache; father, good, subject to headache." 3. "Delicate in early life, one lung seriously affected; mother delicate and phthisical." I can find only two cases, neither very strongly marked, in which both parents were described as unhealthy, although marriages between such persons are not infrequent. These returns seem to show that the issue of such marriages are barely capable of pushing their way to the front ranks of life. All statistical data concur in proving that healthy persons are far more likely than others to have healthy progeny; and this truth cannot be too often illustrated, until it has taken such hold of the popular mind, that considerations of health and energy shall be of recognized importance in questions of marriage, as much so as the more immediately obvious ones of rank and fortune.

Steady perseverance is a third quality on which much stress is laid, but this might have been anticipated, and it is unnecessary to quote instances.

Some prevalence of practical business habits might also have been anticipated, but it proves much more common than I had expected. Among those who have sent me returns, I count no less than seventeen who are active heads of great commercial undertakings. There are also ten medical men in the highest rank of practice, and eighteen others who fill or have filled important official posts. A most eminent biologist wrote as follows, in reply to the inquiry whether he had any special tastes bearing on scientific success, in addition to those for his own line of investigation:—"I have no special talent except for business, as evinced by keeping accounts, being regular in correspondence, and investing money very well." It is clear that method and order are essential to the man who hopes to deal successfully with masses of details.

Next, as regards the more special qualities; those already mentioned, of energy, health, steadiness of pursuit, and business habits being of general application. The first of these is independence of character. Fifty of my correspondents show that they possess it in excess, and in only two is it below par. Here are a few examples:—1. "Left æt. 12" [that is, ran away from] "a school where I had received injustice from the master." 2. "Opinions in almost all respects opposed to those in which I was educated." 3. "I have always taken my own independent line. My heresy prevented my advancement." 4. "Preference for whatever is not the fashion, not popular, not rich, not very able to help itself, yet with qualities unworthily overlooked or unjustly oppressed." The home atmosphere which the scientific men breathed in their youth was generally saturated with the spirit of independence. Examples:—1. "My father was extremely independent, in some respects more so than I am. He never took off his hat to anyone in his life, and never addressed anyone as Esq." 2. "My father was a Liberal when Liberalism (then styled Jacobinism) was highly obnoxious, an early denouncer of slavery and advocate of religious liberty, a free trader when the world was protectionist, and an opponent of unrighteous war when war was most popular. He was for mitigating our criminal code when hanging was regarded as the sheet-anchor, and, in a word, was politically and socially a very independent spirit." In confirmation of the assertion that the scientific men were usually brought up in families characterized by independence of disposition, I would refer to the strange variety of small and unfashionable religious sects to which they or their parents belonged. We all know that Dalton, the discoverer of the atomic theory, and Dr. Young, of the undulatory theory of light, were both Quakers, and that Faraday was a Sandemanian. So I find in these returns numerous cases of Quaker pedigree; and I know of one man, not as yet technically on my list, who was born a Sandemanian. There are also representatives of several other small sects, as Moravians and Bible Christians, and the Unitarians are numerous. It will be understood that the object of saying this is not to throw light on the religious tendencies of the scientific men (concerning which I have much material), because so off-hand a statement would mislead, but to prove that they and their parents had the habit of doing what they preferred, without considering the fashion of the day. The man of science is thoroughly independent in character.

We now come to what I look upon as the salt of the character of most scientific men, namely, strong innate taste for science or for some special branch of it. It is not universal even among those who have had the highest success, but it is very common, and it sometimes attains to the height of a passion which is not transient, but abides. Though decidedly hereditary in numerous cases, its appearance is more capricious than health or energy, and it often happens that the scientific man is the only member of his family in whom the taste has shown itself. The following are a few examples of innate taste:— 1. "Thoroughly innate; I had no regular instruction, and can think of no event which especially helped to develop it. Bones and shells were attractive to me before I could consider them with apparent profit, and I had a fair zoological collection by the time I was 15." 2. "If any tastes be innate, mine were. They date from beyond my recollection. They were not determined by events occurring after manhood, but I think the reverse; they were discouraged in every way." 3. "I should say innate. As to whether they were largely determined by events occurring after manhood, I think not. All I can say is, that neither profession, nor marriage, nor sickness, have been able to affect them." 4. "As far back as I can remember, I loved nature and desired to learn her secrets, and I have spent my whole life in searching for them. While a schoolboy I taught myself . . . under great difficulties."

Let us now put these results together. We have seen that energy, health, steady pursuit of purpose, business habits, independence of character, and a strong innate taste for science, are characteristics of scientific men. Probably one half of the men on my list possess every one of them in a considerable and some of them in a very high degree. If one or more of these qualities be deficient, success becomes impossible, unless its absence is supplemented by other and as yet unclassified conditions. The want of time prevents me from entering into these, and I must postpone further results to a future publication. However, two groups of cases may be specified in which only a few of the above-mentioned qualities are present, and which end in an abortive career. The one is the possession of energy, health, and independence of character in excess, and little else to control them.

These are dangerous gifts. Those who have them are apt to renounce guidances by which the great body of mankind move safely, and to follow out a career in which they are almost certain to blunder and fail egregiously. Probably every large emigrant ship takes out many such men, full of unjustifiable self-confidence, who, to use a current phrase, "knock about in the world," waste their health, youth, and opportunities, and end broken down. Another common group of cases are those where a strong innate taste for science is accompanied by independence of character and steadiness of pursuit, but with no other quality helpful to success, and who therefore fail. There is hardly a village where some ingenious man may not be found who has ideas and much shrewdness, but is crotchety and impracticable. He wants energy and business habits, and so he never rises. There are many who brood over subjects like perpetual motion, whose peculiarities are well illustrated in De Morgan's book of paradoxes. We also frequently meet persons of the stamp that justifies the old-fashioned caricature of scientific men, being absorbed in some petty investigation, utterly deficient in business habits, and noted for absence of mind. I may add that even idiots have often strongly quasi-scientific tastes, as love for simple mechanism, or objects of natural history, and they have a pleasure in collecting. Also, we all know that madmen have often persistency, as shown by their brooding on a single topic.

Lastly, I wish to give some idea of the very general prevalence of mechanical tastes among the scientific men generally. One would have expected to find it among mechanicians and physicists, but it is just as strong among the biologists and others. One chemist made a 12-inch reflecting telescope; two eminent surgeons have an extraordinary aptitude for and love of mechanical manipulation; two very eminent biologists had a passion for it, and both, if they had followed the bent of their own minds, would have been engineers by profession.

All tends to show that the scientific mind is directed to facts and abstract theories, and not to persons or human interests. The man of science is deficient in the purely emotional element, and in the desire to influence the beliefs of others. Thus I find that two out of every ten do not care for politics at all; they are devoid of partisanship. They school a naturally equable and independent mind to a still more complete subordination to their judgment. In many respects their character is strongly anti-feminine. It is a curious proof of this, that in the very numerous answers which have reference to parental influence, that of the father is quoted three times as often as that of the mother. It would not have been the case, judging from inquiries I elsewhere made, if I had been discussing literary men, commanders, or statesmen, or, still more, divines.

I regret much that time makes it impossible for me now to dive deeper into the rich mine of facts contained in my returns. It becomes necessary for me to leave this branch of the subject and to pass on to some interesting considerations regarding the measurement of qualities such as those we have been engaged upon. These considerations are of the most general application, and are as applicable to magnitude as they are to intellect and morals, and to every form of animal or vegetable life as they are to men. I shall therefore speak about the size of nuts, and peas, and acorns, as being easily experimented on, and deduce from these the results which I would fain apply to the moral and intellectual qualities of mankind.

The law of statistical constancy may be taken for granted. It is evidenced by the experience of insurance offices against fire, death, shipwreck, and other contingencies, always with the proviso that the facts are gathered with discretion, on well-known general principles. Hence we may say with assurance, that although two common nuts may differ, yet the contents of different packets, each containing 1000 nuts, will be scarcely distinguishable, for the same number of nuts of different sizes will be found in each. Let the contents of the several packets be each arranged in a long row, in order of size, beginning with the biggest nut and ending with the smallest, and place the rows rank behind rank; then by the law of statistical constancy the nuts in the same *files* will in all cases be closely alike (except the outside ones, where more irregularity prevails). Again, if we incorporate two rows into one of double length, still preserving the arrangement as to regular gradation in size, the centre nuts of the two original series will still be found at or near the centre of the compound series, the nuts in quarter positions will still be in quarter positions, and so on. Hence, whatever be the length of the series the *relative* position in it of the nut will be a *strict criterion* of its size. This is of course equally true of all groups of qualities or characters whatever, in which the law of statistical constancy prevails, the series, in each case, being arranged according to gradations of the quality in question. Each individual is measured against his neighbour, and it is quite unnecessary to have recourse to any external standard. As regards a scale of equal parts, I make use of a converse application of the law of "frequency of error" [this was illustrated by many experiments], which shwos that in a row (say as before) of nuts, if we take those which occupy the three quarterly divisions (1st quarter, centre, 3rd quarter) as three elementary graduations of size, a continuous scale of graduations will be determined by the following series, in which the places of the nuts are supposed to be reckoned from the end of the row where the large nuts are situated, and to be given in per-thousandths of the entire length of the row. It might be called the "Common Statistical Scale" (S. S.). The place of + 4° would be at 4 thousandths from large end; + 3° at 21 thousandths; + 2° at 89; + 1° at 250; 0° at 500; − 1° at 750; − 2° at 911; − 3° at 979; and − 4° at 996, or 4 thousandths from the small end of the row. Thus if we say that the size of a nut is + 2° S. S., we absolutely define, or rather identify, what we are speaking about. Anybody can procure such a nut independently by getting a quart of nuts and arranging them. Also we know that the difference between a nut of + 4° S. S. and + 1° S. S. is 3°, and therefore three times as great as between one of + 2° S. S. and the latter. It cannot be affirmed that this is a precise scale of equal parts for all qualities, but it is found to hold surprisingly well in a great variety of vital statistics; perhaps, too, the mere thickness of tissues may be a chief element in the physical basis of life. This scale appears, at all events, more likely to be nearly approximative to one of equal parts, for qualities generally, than any other that can be specified, and it certainly affords definite standards subject to the law of statistical constancy. The habit should therefore be encouraged in biographies, of ranking a man among his contemporaries, in respect to every quality that is discussed, and to give ample data in justification of the rank assigned to him. By the general use of a system like the above, which is universally applicable, social and political science would be greatly raised in precision.

I now pass on to the education which the scientific men had in their youth, in the hope that my results may give assistance to those who are endeavouring to frame systems of education suitable to the wants of the day. What I have to say, is very partial; it refers solely to the opinions the scientific men entertain of the merits and faults of their own several educations. Their views are remarkably unanimous, considering the very different branches of inquiry they are interested in, and the great dissimilarities in their education. I should mention, that one-third have been educated at Oxford or Cambridge, one-third at Scotch, Irish, or London Universities, and the remaining third have been at no University at all. I am totally unable to decide which of the three groups occupies the highest scientific positions, they seem to me very much alike in this respect. The merits they all ascribe to variety of education are to be gathered from the following examples:—1. "Not tied down to old courses of classics and mathematics." 2. "Sufficient groundwork in many subjects to avoid error." 3. "Early introduced to many subjects of interest." 4. "A well-balanced education, including chemistry, botany, logic, and political economy." 5. "A variety of subjects, and attention to details." 6. "Coming in contact with persons of every rank and sitting in the same form" [in a Scotch school] "with the sons of tradesmen, and ploughmen, as well as gentlemen." In contrast to this, here are some examples as to the faults of their education:—1. "No mathematics, nor modern languages, nor any habits of observation or reasoning." 2. "Enormous time devoted to Latin and Greek, with which languages I am not conversant." 3. "Omission of almost everything useful and good, except being taught to read; Latin, Latin, Latin!" 4. "In an otherwise well-balanced education, three years were spent on Latin and Greek grammar, a blank waste of time." 5. "Neglect of many subjects for the attainment of one or two; not pushing mathematics to a useful end." Evidence such as this, which could be largely added to, establishes the advantage of variety of study. One group of men speak gratefully because they had it, and another group speak regretfully because they had it not. I find none who had a reasonable variety who disapproved of it, none who had a purely old-fashioned education who were satisfied with it. The scientific men who came from the large public schools usually did nothing when there; they could not assimilate the subjects taught, and have abused the old system heartily. There are several serious complaints about superficial and bad teaching which I need not quote. Overteaching is thoroughly objected to; thus, in speaking of merits of education, I find :—1. "Freedom to follow my own inclinations, and to choose my own subjects of study, or the reverse." 2. "The great proportion of time left free to do as I liked, unwatched and uncontrolled." 3. "Unusual degree of freedom." I should add, that there are many touching evidences of the strong effect of home encouragement and teaching. As regards the subjects specially asked for, even by biologists, mathematics take a prominent place. Two of my correspondents speak strongly of the advantages derived from logic, and the weighty judgment of the late John S. Mill powerfully corroborates their opinions. Accuracy of delineation is also spoken of, and, owing to the extraordinary prevalence of mechanical aptitudes, I believe that the teaching of mechanical manipulation would be greatly prized. The interpretation that I put on the answers as a whole, is as follows: To teach a few congenial and useful things very thoroughly, to encourage curiosity concerning as wide a range of subjects as possible, and not to overteach. As regards the precise subjects for rigorous instruction, the following seem to me in strict accordance with what would have best pleased those of the scientific men who have sent me returns :—1. Mathematics, pushed as far as the capacity of the learner admits, and its processes utilized as far as possible for interesting ends and practical application. 2. Logic (on the grounds already stated, but on those only). 3. Observation, theory, and experiment, in at least one branch of science; some boys taking one branch and some another, to ensure variety of interests in the school. 4. Accurate drawing of objects connected with the branch of science pursued. 5. Mechanical manipulation, for the reasons already given, and also because mechanical skill is occasionally of great use to nearly all scientific men in their investigations. These five subjects should be *rigorously* taught. They are anything but an excessive programme, and there would remain plenty of time for that variety of work which is so highly prized, as: ready access to books; much reading of interesting literature, history and poetry; languages learnt, probably best during the vacation, in the easiest and swiftest manner, with the sole object of enabling the learners to read ordinary books in them. This seems sufficient, because my returns show that men of science are not made by much teaching, but rather by awakening their interests, encouraging their pursuits when at home, and leaving them to teach themselves continuously throughout life. Much teaching fills a youth with knowledge, but tends prematurely to satiate his appetite for more. I am surprised at the mediocre degrees which the leading scientific men, who were at the Universities, have usually taken, always excepting the mathematicians. They prefer to fix of their own accord on certain subjects, and seem averse to learn what is put before them as a task. Their independence of spirit and coldness of disposition are not conducive to success in competition, they doggedly go their own way and refuse to run races.

Science has hitherto been at a disadvantage compared with other competing pursuits, in enlisting the attention of the best intellects of the nation, for reasons that are partly inherent and partly artificial. To these I will briefly refer in conclusion, with especial reference to the very important question, as to how far the progress of events tends to counterbalance or remove them.

If we class energy, intellect, and the like, under the general name of ability, it follows that, other circumstances being the same, those able men who have vigour to spare for extra professional pursuits, will be mainly governed in the choice of them by the instinctive tastes of their manhood. The majority will address themselves to topics nearly connected with human interests, a few only will turn to science. This tendency to abandon the colder attractions of science for those of political and social life, must always be powerfully reinforced by the very general inclination of women to exert their influence in the latter direction. Again, those who select some branch of science as a profession, must do so in spite of the fact that it is more unremunerative than any other pursuit. A great and salutary change has undoubtedly come over the feeling of the nation since the time when the present leading men of science were boys, for the state of education was then such as an enemy might have invented on purpose to exterminate science. It crushed the inquiring spirit, the love of observation, the pursuit of inductive studies, the habit of independent thought, and it protected classics and mathematics by giving them the monopoly of all prizes for intellectual work, such as scholarships, fellowships, church livings, canonries, bishoprics, and the rest. This gigantic monopoly is yielding, but obstinately and slowly, and it is unlikely that the friends of science will be able, for many years to come, to relax their efforts in educational reform. As regards the future provision for successful followers of science, it is to be hoped that, in addition to the many new openings in industrial pursuits, the gradual but sure development of sanitary administration and statistical inquiry may in time afford the needed profession. These may, as I sincerely hope they will even in our days, give rise to the establishment of a sort of scientific priesthood throughout the kingdom, whose high duties would have reference to the health and well-being of the nation in its broadest sense, and whose emoluments and social position would be made commensurate with the importance and variety of their functions.

[F. G.]

## GENERAL MONTHLY MEETING,

Monday, March 2, 1874.

**George Busk**, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Capt. Alexander Baillie,
Walter Frederick Ball, Esq.
Thomas R. Bannon, Esq.
The Rev. Fitzwilliam Bowyer,
Mrs. Arthur Brandreth,
Charles James Busk, Esq.
John W. Busk, Esq.
William Henry Challis, Esq.
James Grant Fraser, Esq.
Miss Louisa Garrard,
Miss Harriet Gassiot,
Charles Addington Hanbury, Esq.
William Thomas Fitzherbert Mackenzie Ingall, Esq.
The Hon. Archibald Michie, Q.C.
Capt. W. David Miller
John Samuel Phené, Esq. F.S.A. F.G.S.
Septimus Rivington, Esq.
Augustus Burke Shepherd, Esq.
A. M. Silber, Esq.
Mrs. Charles Southwell,

were *elected* Members of the Royal Institution.

The Special Thanks of the Members were given to **John Browning**, Esq. M.R.I. for his Present of a Browning's Electric Lamp.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

**From**

*The Government of India*—Memoirs of the Geological Survey of India, Vol. X. Part 1. 8vo. 1873.—Records, Vol. VI. Parts 1–4.
Palæontologica Indica. Cretaceous Fauna, Nos. 3, 4. fol. 1873. Jurassic Fauna, Vol. I. No. 1. fol. 1873.

*American Academy of Arts and Sciences*—Memoirs, New Series, Vol. IX. Part 3. 4to. 1873.—Proceedings, Vol. VIII. Nos. 52–62. 8vo. 1872–3.

*American Association for the Advancement of Science*—Proceedings at the 21st Meeting, Aug. 1872. 8vo. 1873.

*Armstrong, Professor H. E. Ph.D. F.C.S.*—Introduction to the Study of Organic Chemistry. 16to. 1874.

*Asiatic Society of Bengal*—Journal, 1873, Part 1, No. 2; Part 2, No. 3. 8vo.
Proceedings, 1873, Nos. 5–8. 8vo.

*Astronomical Society, Royal*—Monthly Notices, Vol. XXXIV. Nos. 1–3. 8vo. 1873.
*Boston Society of Natural History*—Memoirs, Vol. II. Part 2, Nos. 2, 3.
Proceedings, Vol. XIV. Nos. 15–27. Vol. XV. 8vo. 1872–3.
*British Architects, Royal Institute of*—Sessional Papers, 1873–4, Nos. 5, 6. 4to.
*British Museum Trustees*—Guides to Egyptian Room and Slade's Prints. 16to. 1874.
*Chemical Society*—Journal for Dec. 1873, Jan. and Feb. 1874. 8vo.
*Connecticut Academy of Arts and Sciences*—Transactions, Vol. II. Part 2. 8vo. 1873.
*Cunningham, D. Douglas, M.B. (the Author)*—Microscopic Examinations of Air. fol. 1873.
*Editors*—American Journal of Science for Jan. 1874. 8vo.
Astronomical Register for Feb. 1874. 8vo.
Athenæum for Feb. 1874. 4to.
Chemical News for Feb. 1874. 4to.
Engineer for Feb. 1874. fol.
Food Journal for Feb. 1874. 4to.
Journal for Applied Science for Feb. 1874. fol.
Journal of Gas-Lighting for Feb. 1874. 4to.
Nature for Feb. 1874. 4to.
Nautical Magazine for Feb. 1874. 8vo.
Once a Week for Feb. 1874. 8vo.
Pharmaceutical Journal for Feb. 1874. 8vo.
Telegraph Journal for Feb. 1874.
*Franklin Institute*—Journal, Nos. 574–576. 8vo. 1873.
*Geological Institute, Imperial, Vienna*—Jahrbuch, 1873. No. 3. 8vo.
Verhandlungen, 1873. Nos. 11–13.—Abhandlungen, Band VI. Theil 1. fol. 1873.
*Geological Society*—Quarterly Journal, No. 117. 8vo. 1873.
*Linnean Society*—Journal, Nos. 57, 74. 8vo. 1874.
*Meteorological Office*—Quarterly Weather Report, 1873, Part 1. 4to.
*Negretti and Zambra*—Treatise on Meteorological Instruments. 8vo. 1874.
*New York State Library*—Subject Index. 8vo. 1872.
Annual Report. 8vo. 1873.
*Nystrom, John W. Esq. C.E. (the Author)*—Principles of Dynamics. 8vo. 1874.
*Preussische Akademie der Wissenschaften*—Monatsberichte, Sept. Oct. und Dec. 1873. 8vo.
*Rome, Accademia dei Lincei*—Atti, Anno III. 4to. 1873.
*Royal Society of London*—Philosophical Transactions for 1873, Part 1. 4to.
Proceedings, Nos. 147, 148. 8vo. 1874.
*Royal Society of Victoria*—Reports of the Exploration Committee, 1863–72. fol.
*Saxon Society of Sciences, Royal*—Abhandlungen, Band XVI. Nos. 5, 6. Band XVII. No. 1. 4to. 1873.
Berichte: Phil. Hist. Classe, 1872. Math. Phys. Classe, 1872, Nos. 2, 3, 4, 1873. Nos. 1, 2. 8vo.
*Smithsonian Institution*—Smithsonian Report for 1871. 8vo. 1873.
Smithsonian Miscellaneous Collections, Vol. X. 8vo. 1873.
*Spencer, Sawyer, Bird, and Co. Messrs.*—The Autotype Process. Third Edition. 12mo. 1873.
*Statistical Society*—Journal, Vol. XXXVI. Part 4. 8vo. 1873.
*Symons, G. J. Esq. (the Author)*—Symons' Monthly Meteorological Magazine, Feb. 1874. 8vo.
*Taylor, Alfred S. M.D. F.R.S. (the Author)*—Manual of Medical Jurisprudence. Ninth Edition. 16to. 1874.
*United Service Institution, Royal*—Journal, Nos. 75, 76. 8vo. 1874.
Lectures to Volunteer Officers. 8vo. 1873.
*United States Geological Survey*—F. V. Hayden: Montana, Idaho, Wyoming, and Utah. 8vo. 1873.
*Vereins zur Beförderung des Gewerbfleisses in Preussen*—Verhandlungen, Mai bis Oktober. 1873. 4to.
*Wisconsin Academy of Sciences, Arts, and Letters*—Transactions, 1870–2. 8vo.

## WEEKLY EVENING MEETING,

Friday, March 6, 1874.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Sir Samuel White Baker, M.A. F.R.S.

### *On the Suppression of the Slave Trade on the White Nile.*

(Abstract.)

In the work just published on the 'Heart of Africa,' by the distinguished German traveller Dr. Schweinfurth, an excellent account is given of the Central African slave trade, and his views upon the subject coincide with my own opinions of Mohammedan countries. In page 434, vol. ii., he writes:—"But the task is gigantic, and no one can be more sensible of this than the traveller who has lingered at any of the sources of the slave trade. One point there is upon which all are unanimous—that from Islamism no help can be expected, and that with Islamism no compact can be made. . . . . Islamism, the child of the Desert, has everywhere spread desolation, and wherever it has penetrated, deserts have arisen bleak and bare as the rocks of Nubia and Arabia, and under its influence every nation, from Morocco to the Isles of Sunda, has congealed into a homogeneous mass; inexorably it brings all to one level, remorselessly obliterating all traces of nationality or race. . . . . That Islamism is capable of progress is merely a supposition that has been hatched up from books, and has no foundation."

In another portion of his work he remarks:—"The ill-feeling and smothered rage against Sir Samuel Baker's interference, nurtured by the higher authorities, breaks out very strongly amongst the less reticent lower officials. In Fashoda, and even in Khartoum, I heard complaints that we (the Franks) were the prime cause of all the trouble, and if it had not been for our eternal agitation with the Viceroy, such measures would never have been enforced."

In page 477 he continues his remarks:—"Notwithstanding that Sir Samuel Baker was still on the upper waters of the river, the idea was quite prevalent in all the seribas, that as soon as the 'English pasha' had turned his back upon Fashoda (the government station in the Shillook country), the Mudir (governor) would relapse into his former habits, and levy a good round sum on the head of every slave, and then let the contraband stock pass without more ado. But for once the seriba people were reckoning without their host. The Mudir had been so severely reprimanded by Baker for his former delinquencies that he thought it his best policy, for this year at least, to be as energetic as he could in his exertions against the forbidden trade; and his measures were so summary, and executed with such methodical strictness, that unless I had known him I could scarcely have believed him to be a Turk."

I may here mention that this officer, the governor of a large province, was put in irons and disgraced publicly by the Khedive's orders, upon my representation of his slave-hunting.

Quotations from such a traveller as Dr. Schweinfurth are valuable, as he is not a casual visitor who might catch the gossip of a country through which he might pass, but he is a man eminent in science, who as an explorer and professional botanist has been for some years behind the scenes at the very sources of the slave trade.

I have not the pleasure of his acquaintance, but his opinion agrees so thoroughly with my own, that, combined with the reports of well-known travellers who have added their testimony to the painful fact of official connivance with the slave trade, I must beg you to accept without hesitation this assertion:—The Mohammedan officials of Egypt, and the Mohammedan people generally, will return to the slave trade the instant that they may be relieved from the pressure of foreign restraint. The Khedive of Egypt may issue the most stringent orders, but they will be disobeyed, and he will be deceived.

I have stopped the slave trade of the White Nile, but the traffic may and will be resumed should European commanders be withdrawn. Even should the White River remain pure, the slaves will be conveyed across the desert *viâ* Darfur and Kordofan. Large markets will be established to which the traders will concentrate from all parts of Africa to purchase slaves. These will be dispersed in gangs, and be distributed through all the slave-dealing countries of the East.

The governors of Egyptian provinces are to a man in favour of the slave trade; thus the prohibition of slavery is to them a mine of wealth. The law gives to them the power to seize and confiscate all slaves in the hands of dealers. Thus the arrival of a caravan with 500 slaves would be tantamount to a present of 1000*l.* or more to the government official, who would receive a toll of 2*l.* a head, and let them pass free.

There is a simple method in attacking this great evil that would, I am convinced, be eminently successful. It is European influence alone that will effectually suppress the slave trade, and this same influence will alone save Turkey and Egypt from irretrievable ruin.

In a former work, 'The Nile Tributaries of Abyssinia,' I fully exposed the depredations of the soldiery when employed as tax collectors in the Soudan. By over-taxation and pillage by officials the peasantry are literally eaten up. Thousands upon thousands have forsaken the country, and have commenced a life of brigandage as slave-hunters among the negro tribes. Nine years ago, when I was descending the Nile from Khartoum to Berber, a distance of 200 miles by river, the fertile soil on either bank was in the highest state of cultivation. This valuable extent of country was watered by 4000 sakyeers, or water-wheels. By day and night the irrigation was continued, and the discordant hum and creaking of the machines, if disturbing a night's rest, nevertheless assured the traveller that industry was wide awake, and that prosperity would be the reward of labour. When I returned to that same country in January, 1870, I looked for the past scene in vain.

A steamer and a diahbiah were awaiting me at Berber. As we steamed against the strong current for 200 miles to Khartoum I looked with astonishment and dismay upon the country. Now and then a tuft of neglected date palms might be seen; but the river banks, formerly verdant with heavy crops, had become a wilderness. Villages once crowded had entirely disappeared. The population was gone. The night, formerly discordant with the creaking of the water-wheels, was now silent as death. There was not a dog to howl for a lost master. The discord of a water-wheel would to my ears have been harmony. Industry had vanished; oppression had driven the inhabitants from the soil; the most fertile land on earth had been abandoned to hyenas. This was Egyptian rule, and I was on my path to conquer fresh lands for Egypt!

This terrible desolation was caused by the Governor-General of the Soudan, who, although himself an honest man, was a fanatical Mohammedan, who left his territory to the sole care of God. He simply increased the taxes, and trusted in Providence. In one year he sent to the delighted Khedive his master, at Cairo, more than 100,000*l.* in dollars wrung from the poor peasantry of the Soudan. In the following year it was difficult to get change for a sovereign. It must be borne in mind that a tax suddenly imposed in the Soudan that would produce 100,000*l.* surplus revenue, would be in real fact a tax of 200,000*l.*, as an equal amount is always extorted from the peasantry by the collectors.

The population of the richest portions of the Soudan thus abandoned the country, and the greater portion betook themselves to the slave trade of the White Nile, where in their turn they might trample upon the rights of others; where, as they had been plundered, they could now plunder; where they could reap the harvest of another's labour, and where undisturbed they might indulge in the great enterprise of slave-hunting.

Having passed through the deserted country from Berber, I arrived at Khartoum. Nothing was ready for my expedition, but I found that the Governor-General had just prepared a squadron of eleven vessels, with several companies of regular troops, to form a settlement at the copper mines on the southern frontier of Darfur. This expedition had been placed under the command of a man named Kutchuk Ali, who was one of the most notorious ruffians and slave-traders of the White

Nile! Thus, at the same time that the Khedive of Egypt had employed me to suppress the slave trade of the Nile, a Government expedition had been entrusted to the command of a well-known slave-hunter!

This was only one peculiarity in the policy of the Soudan authorities. The great outcry for money had caused an increase of taxation, which, as has been already shown, had caused the flight of large numbers of the population to the White Nile slave parties.

The Governor-General of the Soudan now bethought himself: "By what right do these people make fortunes in unknown lands beyond the upper Nile?" It was easy to understand that they had no right. This was a golden opportunity for the Governor, who accordingly established a tax upon every trader to the White Nile, in the peculiar form of a lease. According to the position and importance of each trader, a lease was made out, by which the Government let to him for a certain term of years an undefined portion of Central Africa which did not belong to Egypt, and over which the Khedive had neither right nor authority. These leases enabled the traders, for the annual payment of several thousand pounds, to establish stations, and engage in their so-called trade in distant lands belonging to individual tribes, where no government was represented, and where an armed and organized Arab force would be able to commit any atrocity at discretion. There is no doubt that the actual wording of the lease was admirable, inculcating moral precepts, and warning the adventurers against a participation in the slave trade; but if the Governor-General or any other authority should presume to declare himself ignorant that the real object of the enterprise was slave-hunting, he is simply stating that which is false.

Now to exhibit a picture of a "house divided against itself," I will quote the leading passages of the instructions I received in writing from the Khedive Ismail Pacha, prior to my departure for the expedition. In these it will be seen that the suppression of the slave trade was the prime object of the enterprise. Thus, while the Khedive of Egypt determines upon a reform which is the first step towards civilization, his chief officer, the Governor-General, is not only conniving at, but actually making revenue out of the slave trade by leasing out Central Africa to slave-hunting companies.

This is the text of the orders I received from the Khedive, dated 19 May, 1869:—

"We, Ismail, Khedive of Egypt,

"Considering the savage condition of the tribes which inhabit the countries in the Nile basin;

"Considering that neither government, nor laws, nor security exist in those countries;

"Considering that humanity enforces the suppression of the slave-hunters who occupy those countries in great numbers;

"Considering that the establishment of legitimate commerce throughout those countries will be a great stride towards future civilization, and will result in the opening to steam navigation of the great equatorial lakes of Central Africa, and in the establishing of a permanent government, . . . . We have decreed and now decree as follows:—

"An expedition is organized to subdue to our authority the countries situated to the south of Gondokoro,

"To suppress the slave trade; to introduce a system of regular commerce;

"To open to navigation the great lakes of the equator;

"And to establish a chain of military stations and commercial depôts, distant at intervals of three days' journey, throughout Central Africa, accepting Gondokoro as the base of operations.

"The supreme command of this expedition is confided to Sir Samuel White Baker for four years, commencing from the 1st April, 1869; to whom also we confer the most absolute and supreme power, even that of death, over all those who may compose the expedition.

"We confer the same absolute authority to him over all those countries belonging to the Nile basin south of Gondokoro."

Can any orders be clearer or more concise? The slave trade is to be suppressed; legitimate commerce is to be introduced; protection is to be afforded by the establishment of a regular government. And to achieve this task an Englishman is selected; and he is armed with a despotic power that never before was entrusted by a Mohammedan to a Christian.

Here lies the difference between the Khedive and his subjects. The one is far from a fanatic; he is a man of the world, intelligent and experienced; but his active energy is always working against a passive resistance.

To suppress the slave trade was to compliment the powers of Europe; to denote the superiority of Egypt over other Oriental nations; to lay the first stone in the foundation of a new civilization; and to save to Africa a population that was now rapidly disappearing. To effect this great reform it was necessary to annex the Nile Basin, and establish a government in countries which had hitherto been without protection, and a prey to the adventurers from the Soudan. To convey steel steamers from England, and to launch them upon the Albert Lake, and thus open up the resources of Central Africa; to establish legitimate trade in a country which had hitherto been a field of rapine and of murder; to protect the weak and punish the evildoer, and to open the road to a great future, where the past had been all darkness and the present reckless spoliation—this was the grand object in which the Khedive was thoroughly sincere.

In this humane enterprise he was firmly supported by his two Ministers, Nubar Pacha and Cherif Pacha (an Armenian and a Circassian). The young princes, his sons, who are well-educated and enlightened men, took the greatest interest in the undertaking, but beyond these few persons all were evil-wishers, who trusted that the expedition would end in failure.

This explanation of the state of feeling in the country, and of the general position of affairs, will be sufficient to prove that there is little hope of success in the Soudan, unless the Khedive shall determine upon the reform I have suggested. If the authority should be vested for a certain term of years in European officers, the sudden change of system would be quickly appreciated by the population. Confidence would be restored by the establishment of an honest government, and the fertile lands that have been forsaken would shortly be restored to cultivation. The beneficial result of this reform would be felt throughout Egypt. European capitalists would invest in Egyptian securities without fear, as they would have confidence in the future development of almost boundless resources.

Few Europeans at present have any idea of the producing power of the Soudan. Those tourists who have been sufficiently hardy to encounter the Nubian deserts can hardly believe that beyond that rainless zone there are lands of extraordinary fertility, only waiting for an opportunity to become one of the most important cotton-producing countries in the world. But what capitalist would be mad enough to invest money in cotton cultivation in Upper Egypt under native administration? Every obstacle would be thrown in his way, and he would be subjected to the passive hostility and petty jealousies and fatal obstructions that have driven every European out of Khartoum, except the Austrian consul and a German tailor, who are now the sole representatives of European civilization.

Formerly there were many European residents and merchants in the Soudan; now there is not one, except the two individuals above mentioned.

It may be easily understood, that where the chief object of the authorities of the Soudan has been to conceal the slave trade, it was their direct policy to get rid of all European witnesses. In this they have thoroughly succeeded. The Khedive is endeavouring to correct this evil generally; and not only has he recalled the former governor-general, but he has altogether abolished the appointment. Instead of a supreme head who directed the Soudan, he has divided this extensive territory into several independent districts, each of which is under the control of an independent governor. All that is now necessary is to place Europeans, properly qualified by their antecedents, in these posts. In five years the country would hardly be recognized.

The great difficulty in all distant inland countries is the want of transport. It is useless to produce, unless there is a means of carrying off to a market the produce of the soil at a remunerative rate. The Soudan would never be fitted for general cultivation unless a railway were established from Khartoum to Cairo. But the Khedive, with his accustomed energy, has already commenced this railway, to which a portion of his new loan was to have been applied. This railway communication will be the key to the development of Central Africa.

The present Khedive of Egypt has taken the lead by introducing all modern improvements for the benefit of his country. I never so much appreciated the electric telegraph as upon my return from Central Africa to Khartoum. I had been two years and five months without any communication, locked up in the very heart of Africa; but not only could I telegraph through 1400 miles of sandy deserts upon my arrival at Khartoum, but I captured my greatest enemy, the chief of the slavers, by telegram at Cairo. This ruffian, who had escaped from me in the upper country, had made a quick camel journey across the desert, but he had forgotten the existence of the telegraph.

Not only has the Khedive established telegraphs which now communicate throughout the distant Soudan, and even with the Red Sea ports, but he has already constructed upwards of 1100 miles of railway throughout Egypt. . . .

When the centurions of Nero first halted at the obstructions and turned back from the insurmountable difficulties of the White Nile, they would hardly have believed that steamers at a future time would traffic upon that desolate river. There are now eleven steamers on the Upper Nile, six of which I sent from Cairo, and one new vessel that I erected at the remote limit of navigation, Gondokoro, N. lat. 4° 54′.

I have remarked elsewhere upon the extraordinary effect produced upon the minds of the natives by a steamer. The vessel launched at Gondokoro was conveyed in sections from England, and most successfully constructed by the shipwrights sent out by Messrs. Samuda's firm. The natives had thus an opportunity of seeing a steamer of 108 tons commenced and completed. They afterwards sailed in her upon her trial trip, and were so interested in the performance of the high-pressure engines, and the general success of the vessel, that the expedition gained more respect by this instance of British superiority than by all our military successes. . . .

The first great step has been taken in the war declared against the slave trade. In the large district under my command that traffic no longer exists, and I entirely purged my territory from the so-called merchants.

The Government has now the entire monopoly of the ivory trade beyond the 5° N. lat. The natives of Unyoro, for the first time introduced to real legitimate trade, were delighted to exchange their ivory for various European manufactures. Manchester goods, cutlery, crockery-ware, and in fact almost all European articles were gladly accepted.

When proper means of transport shall be introduced, the countries between the 4° N. lat. and the equator will be invaluable for the production of coffee. Situated at a mean altitude above the sea level of about 3800 feet, with a wonderfully fertile soil, combined with an agreeable climate and a regular season of rains extending over nine or ten months of the year, there could not be a more favourable position for coffee cultivation. The native population is large, and now that they are protected by the Government they would work for wages. The lower grounds, especially that important country inhabited by the

Shillooks, between the 9° and 12° of latitude, is specially adapted for the cultivation of cotton. The plant is indigenous throughout those districts, and the cotton is spun into threads and formed into a fringe by the natives to form aprons for their women. The Shillooks are excellent people; the tribe numbers above a million, and they are industrious agriculturists. It was in that country that I first liberated cargoes of slaves, and insisted upon the Governor returning to their homes all those whom he had kidnapped. It was most touching to witness the effect of this sudden liberation upon the crowd of unhappy women and children, who, tied neck to neck by ropes, sat huddled together under the charge of the Government soldiers. At first they could not believe in the truth of my order, and did not realize their freedom; but when I insisted upon their liberation, and myself started them on their homeward march, their joy knew no bounds.

I formed an encampment with 1500 men, and remained in the Shillook country seven months. During this time I never had to complain of the people, and I was much struck with their honesty. Women were extremely scarce in my expedition; thus my troops experienced great difficulty in grinding their corn. It was accordingly their habit to entrust the corn to the Shillook natives, who took it to their villages to grind, and after a day or two they returned with the flour. I never had a complaint of deficiency of measure; and it should be explained that corn was scarce in the neighbourhood, owing to the raids that had been made by the Governor of Fashoda; thus it might have been natural that the Shillooks would steal a portion from that entrusted to them.

The fact of my having liberated the slaves was acknowledged by the gratitude of the tribe throughout, and the news of this emancipation spread through the most remote lands, as described by Dr. Schweinfurth in his recent work.

The Shillooks are now producing cotton on an extended scale. They exchange the raw produce for Manchester goods, and there is the prospect of general prosperity, provided the Government remains honest. I introduced the seeds of the finest quality of cotton from Egypt, which throve exceedingly.

The capital town of the Shillook is Fashoda. This is a fortified post garrisoned by two Egyptian regiments. It completely dominates the White Nile, and it would be absolutely impossible for slave-traders to pass if the Governor did his duty.

As I have before stated, I have no confidence in Egyptian officers. The most striking proof of their connivance at the slave trade was afforded a few months ago on my homeward route. As the new steamer was towing my diahbeeah down the White Nile, I overtook three vessels belonging to the principal trader, Agād, with seven hundred slaves on board! Thus, after all that I had done in suppressing the slave trade in the distant south, the traders were actually presuming to pass the Government station of Fashoda, thus proving that there was no danger from the vigilance of the officials.

Upon my arrival at Fashoda, where two large steamers were lying, I gave the necessary orders to the Governor, whom I held responsible for the capture of the three slavers. In a few days they were taken, with 600 slaves on board, about 100 having perished by the small-pox since I parted from them.

The Governor of Fashoda was a new man, only recently appointed; but I had satisfactory proofs that during my absence of some years in the interior, the former governor had been in the habit of pocketing a toll per head for each slave, and asking no further questions. There can be no excuse for the authorities if a slave vessel descends the White Nile.

The present difficulty lies at Darfur, the Sultan of which country is a fanatical Mohammedan and slave dealer. Darfur is the great emporium of the slave trade, and I heard with pleasure within the last few days that the Sultan has had the audacity to invade the frontier and attack the Egyptian forces—in which attack he suffered a severe defeat. This will end in the annexation of Darfur by Egypt, which (always providing that *European* officers are to command) will be the most fatal blow to the Central African slave trade.

If the English Government is serious in the desire to suppress the slave trade, the time has arrived for action. It will not be effected by empty preaching to Oriental powers.

My task has been arduous and thankless as an Englishman and Christian employed by a Mohammedan Government to stamp out a cherished Mohammedan institution. It was natural to expect every species of intrigue and opposition. This has been used unsparingly, which has led to much difficulty and ended in much bloodshed in literal self-defence. I trust, however, that the expedition I have lately concluded will be the foundation for a great future. I most thoroughly cleansed the White River from the slave trade, and established a government in the heart of Africa that was respected and loved by the inhabitants. It remains for others to continue and to improve the work I have done. I can only trust that by overcoming the difficulties and opposition that I met with, I may have cleared and made easy the path for my successor, an officer well known by reputation, who will do all that is possible for British energy to accomplish.

In taking leave of Africa I felt some pangs of regret that my time was too limited to permit me to see good fruit as the result of my labour; but I saw enough in the gratitude of the native population to convince me that if Englishmen take the work earnestly in hand, Africa will at length emerge from the barbarism which has disgraced her.

The Khedive has taken the work most earnestly in hand, and having now retired from his service, I with pleasure acknowledge the kindness that I have always received at his hands and from the principal members of his family. I feel convinced that no one more sincerely regretted the difficulties and the opposition I encountered than Ismail Pacha, the Khedive of Egypt, who, on my return to

Khartoum, was the first to congratulate me by telegram on my success.

Although in the recent expedition I have been the active instrument, the great credit and honour are due to the Khedive of Egypt, who is the first Mohammedan ruler who has dared to strike at the greatest of Mohammedan abominations.

[S. W. B.]

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## WEEKLY EVENING MEETING,

Friday, March 13, 1874.

Sir Frederick Pollock, Bart. M.A. Manager, in the Chair.

### Dr. C. R. Alder-Wright, F.C.S.

### *On the Chemical Changes accompanying the Smelting of Iron in Blast Furnaces.*

Notwithstanding that the operation of iron-smelting is one dating from a very early epoch, and in spite of its magnitude at the present day, and the consequently numerous series of observations and experiments made thereon by various chemists, it is nevertheless true that until recently our knowledge of the chemical changes which occur during the process was extremely limited.

It will not be necessary for me to-night to enter into any detailed description of the apparatus and machinery now in use in this manufacture; the blast furnace, as you are all aware, is virtually a gigantic vertical tube into the top end of which the materials used are continually inserted; these consist of the ore to be smelted, the fuel, and lime or limestone as a flux. At the lower end of the tube air usually heated to 300°–500° is continually injected by a blowing engine through nozzles termed *tuyeres*, whereby the fuel is burnt and the necessary heat generated: the earthy materials of the ore and the lime of the flux unite, forming a fusible "slag," which is continually drawn off at the base, whilst the molten reduced iron accumulates below the slag, being specifically heavier, and is drawn off from time to time into moulds of sand, when it constitutes the pig iron of commerce.

Ordinarily the air injected, or "blast," is previously heated by passing through tubes of iron or piles of brickwork, themselves heated by the combustion of the waste gases that escape from the furnace itself; to collect these, the top of the furnace is closed by a bell-shaped valve which serves for the introduction of materials when open, but which when closed compels the gases to issue through an orifice in the side of the furnace near the top, whence they are led away through pipes to the heating stoves, furnaces of boilers, &c.

The nature of the ore used necessarily exerts a great influence on the chemical changes that take place; the following Table indicates the general composition of several of the chief descriptions of ore used. In some instances the iron exists naturally as ferric oxide; in other cases chiefly as ferrous carbonate.

| | Hæmatite. | Magnetic Ore. | Spathic Ore. | Clay Ironstone. | Brown Ore. |
|---|---|---|---|---|---|
| $Fe_2O_3$ .. .. | 90–100 | 30–70 | 0–3 | 0–3 | 40–70 |
| $FeO$ .. .. | .. | 13–33 | 36–50 | 35–55 | 0–5 |
| $Al_2O_3$ .. .. | 0–2 | 0–5 | 0–2 | 1–7 | 1–7 |
| $CaO$ .. .. | 0–3 | 0–5 | 0–4 | 1–14 | 1–7 |
| $MgO$ .. .. | 0–1 | 0–2 | 0–4 | 1–9 | 0–2 |
| $MnO$ .. .. | 0–1 | 0–1 | 1–25 | 0–2 | 0–3 |
| $SiO_2$ .. .. | 0–10 | 0–10 | 0–5 | 2–17 | 1–35 |
| $CO_2$ .. .. | 0–1 | 0–10 | 37–42 | 22–37 | 0–5 |
| $P_2O_5$ .. .. | 0–1 | 0–2 | trace | 0–2 | 0–2 |
| $SO_3$ .. .. | 0–1 | trace | ” | 0–3 | trace |
| $H_2O$ .. .. | 0–1 | 0–4 | — | 0–1 | 6–18 |
| Essential Composition .. | $Fe_2O_3$ with little or no earthy matter. | $Fe_2O$ and $FeO$ with earthy matters. | $FeCO_3$ and $MnCO_3$ crystalline. | $FeCO_3$ and much earthy matter. | Hydrated $Fe_2O_3$ and earthy matters. |

When ferrous carbonate constitutes the ore used, the smelting process is usually facilitated by submitting it to a previous calcination in a kiln like a lime-kiln, whereby the ferrous carbonate becomes transformed into ferric oxide; when, however, the carbonate is not calcined before use, it speedily loses the carbon dioxide present therein, leaving behind an oxide of iron; this occurs in the top portion of the furnace, so that virtually the ores used may be regarded as consisting essentially of some form of iron oxide, with a varying amount of earthy matter of variable kind intermixed therewith.

The fuel used is ordinarily coke or charcoal; when, however, raw coal is employed, it is completely coked in the top portion of the furnace, so that the fuel burnt at the tuyeres is invariably carbon.

As the blast enters the furnace it comes in contact with a mass of incandescent carbon, whereby its oxygen is firstly converted into carbon dioxide, and nextly into carbon oxide, the nitrogen for the most part remaining unchanged; not impossibly, a good deal of the carbon is burnt directly to carbon oxide, without passing through the intermediate stage of carbon dioxide; any moisture present in the blast is similarly converted into a mixture of carbon oxide and hydrogen; the amount of the latter necessarily varies with the hygrometric state of the atmosphere. Although hydrogen is a powerful reducing agent, the influence of the small quantity present in the blast-furnace gases appears to be practically *nil:* the gases issuing at the top of the furnace contain on an average about as much free hydrogen as is brought in by the blast, whence it is evident that the hydrogen has contributed little or nothing to the reduction of the ferric oxide; and again, practical experience shows that the more moisture enters the furnace at the base, the more fuel is requisite to do the work, *i.e.* the development of hydrogen is injurious to the action rather than auxiliary.

What I shall have the honour of bringing before you to-night is the history of the changes that occur as the mixture of carbon oxide and nitrogen formed in the vicinity of the tuyeres rises through the furnace. This action has usually been described hitherto as a very simple one; the carbon oxide being viewed as simply removing the oxygen from the higher oxide of iron, forming successively a lower oxide and metal, carbon dioxide being evolved in accordance with equations (1) and (2), *infra.*

TABLE of CHEMICAL CHANGES taking place in different parts of the BLAST FURNACE.

[A] Reduction of higher oxide to lower oxide and metal by gaseous carbon oxide.

$$\text{(1)}\quad Fe_xO_y + CO = Fe_xO_{y-1} + CO_2$$
$$\text{(2)}\quad Fe_xO_y + yCO = xFe + yCO_2$$

[B] Oxidation of metal to lower oxide and higher oxide by carbon dioxide.

$$\text{(3)}\quad xFe + yCO_2 = Fe_xO_y + yCO$$
$$\text{(4)}\quad Fe_xO_{y-1} + CO_2 = Fe_xO_y + CO$$

[C] Reduction of carbon oxide to carbon.

$$\text{(5)}\quad xFe + yCO = Fe_xO_y + yC$$
$$\text{(6)}\quad Fe_xO_{y-1} + CO = Fe_xO_y + C$$

[D] Reaction of reduced carbon on iron oxide forming carbon dioxide.

$$\text{(7)}\quad 2Fe_xO_y + C = CO_2 + 2Fe_xO_{y-}$$
$$\text{(8)}\quad 2Fe_xO_y + yC = yCO_2 + 2xFe$$

[E] Reaction of carbon dioxide on reduced carbon.

$$\text{(9)}\quad CO_2 + C = 2CO$$

The changes that really take place are, however, much more complicated; it is true that these reactions do take place, but firstly carbon dioxide has a tendency to act on metallic iron, re-forming an oxide, and re-producing carbon oxide by reactions that are just the converse of (1) and (2), *viz.* in virtue of equations (3) and (4).

Nextly, metallic iron acts on carbon oxide, setting free carbon and forming successively a lower and a higher oxide in virtue of equations (5) and (6).

Again, the carbon deposited in virtue of reactions [C] reacts on the higher oxides of iron, producing lower oxides and metal, with evolution of carbon dioxide, in accordance with equations (7) and (8). And finally, carbon dioxide reacts on the deposited carbon, giving rise to the formation of two proportions of carbon oxide in virtue of equation (9).

Changes [A] and [D] may be referred to as *iron-reducing*, [B] and [C] as *iron-oxidizing:* changes [C] may be termed *carbon-reducing*, [D] and [E] *carbon-oxidizing.*

At any given point in the blast furnace all these chemical tendencies are at work simultaneously with more or less vigour according to circumstances; the action actually taking place at this point is, therefore, that due to the single resultant of all the multifarious forces at work; before, therefore, it is possible to enter into any explanation of the chemical changes occurring in the furnace itself, some knowledge must be gained of the relative magnitudes of these several forces. Considerable additions to our knowledge on these points have recently accrued, chiefly through the labours of I. Lowthian Bell, Esq. M.P. with whom I had the pleasure of co-operating in his researches.

As regards tendencies [A]. Although carbon oxide exerts no action on ferric oxide at ordinary temperatures, yet its influence is appreciable at temperatures far below the limit of the mercurial thermometer; the precise point at which the action commences varies with the physical condition of the ferric oxide: thus the following Table illustrates this point:—

TEMPERATURE at which CARBON OXIDE begins to reduce FERRIC OXIDE.

| Substance. | Minimum Temperature at which Carbon Dioxide is formed. | Temperature at which action is well marked. |
|---|---|---|
| | ° | ° |
| Precipitated by ammonia .. .. .. .. .. | 141 | 149 |
| Calcination of ferric nitrate .. .. .. | 145 | 154 |
| Average calcined Cleveland ironstone .. | 199 | 210 |
| Ditto, more highly calcined .. .. .. | 200 | 206 |
| Calcination of ferrous sulphate .. .. | 208 | 216 |
| Pumice-stone soaked in ferrous sulphate solution and calcined .. .. .. .. | 211 | 227 |

The fact that the action is perceptible at so low a temperature may be conveniently illustrated by passing a current of pure carbon oxide over precipitated ferric oxide in a flask heated to 150° in a paraffin bath, and passing the gases subsequently through baryta water, when the formation of a turbidity denotes the production of carbon dioxide. (*Experiment performed.*)

It is noticeable that a specimen of ferric oxide that has been already slightly reduced in this way is first acted upon only at a higher temperature than that required in the first instance.

As regards equation (2) it may be remarked that it is impracticable to reduce *the whole* of any sample of iron oxide by means of gaseous carbon oxide at any temperature short of a white heat; in fact, metallic iron decomposes carbon oxide, becoming itself more or less oxidized at all temperatures below this in virtue of equation (5).

In reference to the converse changes [B], the oxidizing effect of carbon dioxide on iron is scarcely appreciable at a temperature of 300°, but is noticeable at 400° and upwards, being very considerable at about 600°: this may be illustrated by passing a current of carbon dioxide over fragments of metallic iron in a tube heated to a bright red heat, when much carbon oxide is evolved, so that the issuing gases may be inflamed. (*Experiment performed.*)

The physical condition of the iron employed necessarily exerts a considerable influence on the rapidity of the reaction, the more finely divided the metal, the more energetic being the action; thus pyrophoric iron is readily acted on by the air at the ordinary temperature, whilst more massive fragments of metal are unaffected under the same circumstances.

It is noteworthy that carbon dioxide is not capable of acting on all metals alike in this way. Copper, for instance, is unaffected at a red heat or upwards by carbon dioxide, whilst nickel and cobalt are oxidized with the production of carbon oxide, the change, however, taking place with less ease than with pure iron. The following Table illustrates this point.

COMPARATIVE ACTION of CARBON DIOXIDE on IRON, NICKEL, and COBALT, at about 600°.

| | |
|---|---|
| Iron .. .. .. .. .. .. | 100 |
| Cobalt .. .. .. .. .. | 44 |
| Nickel .. .. .. .. .. | 11 |

| | |
|---|---|
| Iron first begins to act .. .. | below 420° |
| Cobalt " " .. .. | about 550° |
| Nickel " " .. .. | above 550° |

The consideration of the causes that lead to the possibility of the occurrence of the inverse reactions [A] and [B], and the different behaviour of iron and copper towards carbon dioxide, introduce us to some general principles in chemical dynamics. Whenever a body unites with oxygen, heat is evolved; thus the following Table illustrates the quantity of heat produced by the union of 16 parts by weight of oxygen with various substances, the materials and products being all viewed as being examined at the constant temperature 500° C.

The numbers are calculated from the so-called "heats of combustion" of the bodies (at 15°) by means of the formula

$$H_{500} = H_{15} + h_1 + h_2 - h_3,$$

where $H_{500}$ = the heat of combustion at 500°.
$H_{15}$ = " " at 15°.
$h_1$ = the heat required to raise the combustible from 15° to 500°.
$h_2$ = " " the oxygen through the same range.
$h_3$ = " " the product of combustion through the same range.

| Combustible. | Product of Combustion. | Kilogramme heat units evolved at 500°. |
|---|---|---|
| Iron .. .. .. | Magnetic (and probably any other) oxide * .. | 66·7 |
| Carbon oxide .. | Carbon dioxide .. .. .. .. .. .. .. .. | 68·8 |
| Carbon .. .. | Carbon oxide .. .. .. .. .. .. .. .. | 27·4 |
| Carbon .. .. | Carbon dioxide .. .. .. .. .. .. .. .. | 48·1 |
| Hydrogen .. | Steam .. .. .. .. .. .. .. .. .. .. | 58·9 |
| Copper .. .. | Cuprous<br>Cupric } oxide*.. .. .. .. .. .. .. .. | 37·6 |

The evolution of heat during these actions may be exemplified by sprinkling iron filings into a large gas-blowpipe flame, when a brilliant shower of sparks is produced, heat and consequently light being developed by the oxidation of the iron. (*Experiment performed.*) Similarly by burning carbon oxide at a jet through which a stream of oxygen can be forced, a blowpipe flame is produced equal in heating power to the oxyhydrogen flame, and like it capable of igniting lime, and thereby giving off a bright light, melting platinum, steel, &c., &c. (*Experiment performed.*) Although carbon oxide actually gives out more heat than hydrogen in uniting with a given weight of oxygen (68·8 kilogramme units as against 58·9 when the action takes place at 500°), it is nevertheless much more difficult to obtain the same calorific effects from a carbon oxide-oxygen flame than from an oxy-hydrogen flame. With a small flame, platinum may be readily fused and lime heated to intense brilliancy with either; but the latter gives a much more compact, and therefore hotter, flame when larger amounts of gas are used: the comparative slowness with which the carbon oxide oxidizes is illustrated by the circumstances that a jet of this gas issuing under some slight pressure from a narrow orifice will not continue to burn in the air, the flame as it were blowing itself out; whilst a jet of hydrogen or of coal-gas burns readily under the same circumstances.

It seems to be a general rule that no reaction of replacement coming under the general form

$$x + yz = y + xz$$

can take place if the reaction is attended with any considerable absorption of heat; *i. e.* the heat given out by the substances $x$ and $y$, in uniting with the same weight of $z$, must either be approximately equal, or else that given out by $x$ in uniting with $z$ must be greater than that evolved by $y$ in combining with $z$. Some few exceptions to this rule are known, notably reactions [D] and [E]: generally, however, the converse is the case, *i. e.* in reactions of this class there is usually an evolution of heat.

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* When a body forms more than one *solid* oxide corresponding to a class of stable well-defined salts, the heat produced for a given weight of oxygen consumed is identical with each oxide; this is not the case when *gaseous* oxides are formed as with carbon; nor even when solid oxides are formed when they do not correspond to classes of stable well-defined salts, *e.g.* some of the oxides of manganese.

Now copper and carbon oxide are not thermally equal as regards combination with oxygen; the latter evolves nearly double the heat given out by the former in uniting with a given weight of oxygen: hence the action of carbon oxide on copper oxide, producing metallic copper and carbon dioxide, must be attended with a large evolution of heat; thus at 500°:—

| | |
|---|---|
| Heat produced by oxidation of carbon oxide to dioxide | + 68·8 |
| Heat absorbed during the reduction of copper oxide .. | − 37·6 |
| Heat evolved .. .. .. .. .. | + 31·2 |

Carbon oxide, therefore, reduces copper oxide to metal, forming carbon dioxide in accordance with the above rule; whilst the converse reaction, *viz.* oxidation of metallic copper by carbon dioxide and conversion of the latter into carbon oxide, does not take place, as it would be attended with a large absorption of heat, *viz.* 31·2 kilogramme units.

The evolution of heat during the reaction of carbon oxide on copper oxide, may be readily illustrated thus:—A tube filled with lumps of copper oxide is heated to a temperature short of visible redness, and a stream of carbon oxide is passed through it; the copper oxide immediately begins to glow brightly from the heat evolution that ensues during the reduction of the metallic oxide: on leading oxygen through the tube, when the reduction is over, and the mass has cooled down so as to be no longer visibly red hot, a still brighter glow is produced from the heat evolution during the reoxidation of the metal: the glowing is less bright in the first instance for the threefold reason that there is somewhat less heat evolved (31·2 units as against 37·6), whilst the resulting carbon dioxide escapes while very hot, and consequently removes heat; again, it dilutes the carbon oxide and makes its action less energetic. (*Experiments performed.*)

In reference to the general rule just referred to relating to the connection between the heat evolution on combination and the relative actions of bodies on one another, one particular case is noteworthy, of which reactions [A] and [B] afford a good example: if in a reaction of the form

$$x + yz = y + xz$$

there is little or no evolution of heat (*i. e.* if $x$ and $y$ both evolve approximately the same amount of heat in uniting with the same weight of $z$), it often happens that the converse reaction

$$y + xz = x + yz$$

may be brought about by modifying the circumstances, so that the following is the general rule for all cases as regards the equation

$$x + yz = y + xz.$$

Case (1).—$x$ evolves more heat in uniting with $z$ than $y$ does.

The reaction takes place under suitable conditions.

Case (2).—$x$ and $y$ evolve approximately the same amounts of heat in uniting with $z$.

The reaction may take place or its converse, according to circumstances.

Case (3).—$x$ evolves considerably less heat than $y$ in uniting with $z$.

The reaction (usually) does not take place, but the converse reaction frequently can occur under suitable conditions.

Thus in the case of reactions [A] and [B], there is but little heat evolution or absorption in either case.

| | | |
|---|---|---|
| [A] | Heat evolved by oxidation of carbon oxide to dioxide | + 68·8 |
| | Heat absorbed by reduction of iron oxide .. .. .. | − 66·7 |
| | Heat evolved .. .. .. .. | + 2·1 |

| | | |
|---|---|---|
| [B] | Heat evolved by oxidation of iron .. .. .. .. .. | + 66·7 |
| | Heat absorbed by reduction of carbon dioxide to oxide | − 68·8 |
| | Heat absorbed .. .. .. .. | − 2·1 |

In this respect reactions [A] and [B] may be compared with another well-known pair of inverse reactions, *viz.* the oxidation of iron by passing steam over it, hydrogen being evolved; and the reduction of iron oxide by passing hydrogen over it, steam being produced. In each of these cases, also, the thermal disturbance is not great, *i. e.* hydrogen and iron are thermally approximately equal in reference to their combination with oxygen at 500°.

Steam and Iron.

| | |
|---|---|
| Heat evolved by oxidation of iron .. .. .. .. .. | + 66·7 |
| Heat absorbed by reduction of steam .. .. .. .. .. | − 58·9 |
| Heat evolved .. .. .. .. | + 7·8 |

Hydrogen and Iron Oxide.

| | |
|---|---|
| Heat evolved by oxidation of hydrogen .. .. .. .. | + 58·9 |
| Heat absorbed by reduction of iron oxide .. .. .. | − 66·7 |
| Heat absorbed .. .. .. .. | − 7·8 |

From the existence of the inverse reactions [A] and [B] it results that when a mixture of the two oxides of carbon acts either on metallic iron or on iron oxide, oxidation of the one or reduction of the other takes place until a definite relation exists between the iron and oxygen present in the resulting substance and the two oxides of carbon in the gaseous mixture; the composition of the inert substance necessarily varies with the temperature and the nature of the mixture of carbon oxides used; the higher the temperature and the more carbon dioxide is present, the larger is the amount of oxygen present in the inert substance. Thus Bell obtained the following numbers:—

| $CO_2$ per 100 volumes of CO. | Temperature. | Oxygen in inert Oxide, $Fe_2O_3 = 100$. | Carbon deposited per 100 of Fe. |
|---|---|---|---|
| 600 | Red heat .. .. .. | 90·0 | nil |
| 100 | Bright red heat .. .. | 67·5 | „ |
| 47 | Full red heat.. .. .. | 8·2 | „ |
| 11 | Approaching whiteness | 11·9 | „ |
| Nil (pure CO) | Bright red heat .. .. | 1·0 | 0·3 |

Reactions [C]. The oxidation of carbon to carbon oxide evolves less heat than the conversion of iron into iron oxide; hence it might be inferred that iron will probably decompose carbon oxide with the evolution of heat, carbon being set free and an oxide of iron formed; thus thermally

| | |
|---|---|
| Heat evolved by oxidation of iron .. .. .. .. .. .. | + 66·7 |
| Heat absorbed by reduction of carbon oxide to carbon .. | − 27·4 |
| Heat evolved .. .. | + 39·3 |

The circumstances attending this change have been studied at great length by Bell; at temperatures not higher than 200° it is possible to obtain small quantities of deposited carbon in virtue of these reactions; but the action is at a maximum at about 420°–450°; at higher temperatures but little carbon can be obtained in this way, owing to the carbon-removing influences [D] and [E]; a few tenths per cent. of carbon, however, are always deposited, the same result being arrived at whether metallic iron or ferric oxide be employed in the first instance. The oxygen communicated to the iron is for the most part removed again in virtue of reactions [A] and [D]; a minute quantity, however, is retained at all temperatures up to about 1000°. Since carbon is also present in the product, it might be supposed that the oxygen is present as occluded carbon oxide; that this is not the case, however, is shown by the circumstance that the whole of the carbon present is left behind in the free state on dissolving the impregnated iron in iodine water, or copper sulphate solution. Moreover, the oxygen present is, under these circumstances, left behind in the form of an oxide of iron.

The process of acieration depends in the first instance on the occurrence of reactions [C]; carbon oxide is absorbed by the bars of iron, and is decomposed, particles of carbon being deposited and iron oxide formed, this latter being immediately reduced again, either by carbon oxide (reactions [A]), or by acting on a part of the deposited carbon (reactions [D]); the blisters on blister steel are probably produced by the attempts at egress of the carbon dioxide thus produced.

When ferric oxide is exposed to the action of carbon oxide at about 420°, the time required to produce a given amount of carbon deposition varies greatly with the physical structure of the iron oxide, probably because this physical structure influences, as above described, the rate at which iron and lower oxides are formed in virtue of reactions [A]: thus the following Table indicates the relative amounts of carbon deposited from various kinds of ferric oxide, by simultaneous exposure to the action of carbon oxide at 420°, for seven hours:—

| | Carbon deposited per 100 of iron. |
|---|---|
| Calcined ferric nitrate | 144·0 |
| Precipitated by ammonia | 95·4 |
| Calcined ferrous sulphate | 54·5 |
| Pumice-stone soaked in ferrous sulphate and calcined | 14·9 |
| Calcined Cleveland ironstone | 0·3 |

After ten or twelve hours, however, the discrepancy is not so marked, as much carbon being then deposited with the Cleveland ore as is with the precipitated oxide in seven hours.

The action of carbon oxide on certain other metals and their oxides has also been examined by Bell; the chief results arrived at are included in the following Table:—

COMPARATIVE ACTION at 400°–500° of CARBON OXIDES on various METALS and OXIDES.

Higher oxides of iron, nickel, and cobalt;
    Reduced to lower oxides, and partially to metal; suboxides ($Fe_2O$, $Ni_2O$, and $Co_2O$) being formed, and much carbon deposited.
Spongy metallic iron, nickel, and cobalt;
    Partially oxidized, much free carbon being formed; iron most active, nickel least.
Higher oxides of manganese;
    Reduced only to monoxide ($MnO$); no free carbon being deposited.
Oxides of copper and lead;
    Reduced to metals; no free carbon deposited.
Oxides of zinc, chromium, and tin; metallic copper, lead zinc, and tin;
    No effect.

In each one of these cases, where any action at all ensues, it is uniformly of such a nature as to be included in the general rule above laid down; thus nickel and cobalt give out approximately as much heat in uniting with 16 grammes of oxygen as does iron, and therefore behave in an analogous fashion, giving rise to converse reactions analogous to [A] and [B].

Reactions [D]. The occurrence of these reactions may be readily demonstrated by heating to redness an iron tube containing a mixture of carbon (as free as possible from hydrogen) and ferric oxide. As soon as the temperature rises to about 400°, carbon dioxide is evolved. The reaction, however, is complicated by the circumstance, that the evolved carbon dioxide reacts on both the carbon present and the iron or lower oxides of iron formed by reactions [D], so as to give rise to the production of carbon oxide by reactions [E] and [A] respectively; hence, as the action goes on, the amount of carbon dioxide in the mixture gradually lessens, and finally little but carbon oxide is evolved. (*Experiment performed.*)

It appears exceedingly probable that the so-called "occluded carbon oxide" contained in malleable iron is really not occluded at all, but is produced in virtue of these reactions from a mixture of carbide and oxide of iron; as already stated, on exposing pure metallic iron to the action of pure carbon oxide at a bright red heat, a substance is obtained containing both carbon and oxygen, *not* in the form of occluded carbon oxide. On heating this product, in a Sprengel vacuum, a mixture of carbon dioxide and carbon oxide is evolved (the latter predominating greatly).

The whole process of "puddling" depends on the occurrence of reactions [D]; the carbon present in the iron (together with other impurities) is thus eliminated by the reaction on it of iron oxides formed in the puddling furnace by the action of the heated air.

Reactions [D] are thermally abnormal; although carbon displaces iron from its oxide, the reaction is attended, not, as might be supposed, with an evolution of heat, but with a considerable absorption; thus at 500°,

| | |
|---|---|
| Heat evolved by oxidation of carbon to carbon dioxide .. | + 48·1 |
| Heat absorbed by reduction of iron oxide .. .. .. .. .. | − 66·7 |
| Heat absorbed .. .. | − 18·6 |

Reaction [E] is also abnormal, being attended with a still greater absorption of heat: thus at 500°,

| | |
|---|---|
| Heat evolved by oxidation of carbon to carbon oxide .. | + 27·4 |
| Heat absorbed by reduction of carbon dioxide to carbon oxide .. .. .. .. .. .. .. .. .. .. .. .. | − 68·8 |
| Heat absorbed .. .. | − 41·4 |

The physical structure of the carbon used has a considerable influence on the temperature at which both reactions [D] and [E] first take place; the lighter and more porous, the more readily is the carbon acted on; that deposited by reaction [C] is especially easily acted on; with ordinary charcoal or coke, reaction [E] does not appear to take place to any appreciable extent below 400°.

It is noteworthy that in some other cases besides reactions [D] and [E], where *solid* carbon is concerned, abnormal results are found to be produced; thus, by the action of carbon oxide on metallic zinc or tin, carbon should be set free even more readily than by iron, nickel, or cobalt, the oxidation-heat of each of the first two metals (per 16 grammes of oxygen) being greater than that of either of the latter three: in point of fact, however, no change at all ensues.

The gases of the blast furnace contain not merely carbon oxide and carbon dioxide, but also a large bulk of nitrogen; all experience, however, goes to prove that this acts solely as a diluent, and in no way interferes with the above-described reactions, saving in delaying them, *i.e.* causing more time to be required for their performance; the presence of nitrogen is, however, of great importance as regards another set of chemical reactions taking place in the lower portions of the furnace.

It has long been known that when nascent potassium or sodium vapour finds itself simultaneously in contact with carbon and nitrogen, the three elements combine, forming a metallic cyanide; the following experiment illustrates this fact: a small charcoal furnace is fed with charcoal previously soaked in potassium carbonate solution and dried; a strong blast of air is then injected into the lower part of the furnace, all apertures being well closed up with clay: when the temperature attains a high enough degree cyanide of potassium is formed,* and escapes to some extent along with other potassium compounds in the form of white fumes carried up by the escaping gases: by diverting these gases through a lateral tube into a vertical tower filled with flints, the solid fume is to a great extent deposited on the surface of the flints; by pouring water down the tower, the fume is dissolved and a solution of potassium cyanide (*inter alia*) trickles out at the base of the tower: on filtration and testing with ferrous and ferric salts and hydrochloric acid, this liquor gives an abundant prussian-blue reaction, showing the presence of much potassium cyanide. (*Experiment performed.*)

The cyanide thus formed acts on the last portions of unreduced oxide of iron, converting it into metal and becoming itself changed to cyanate; at the high temperature of the furnace near the tuyeres this cyanate is probably decomposed with the formation of an alkaline carbonate, and the elimination of nitrogen: a portion of the alkaline carbonate is again converted into cyanide; the majority, however, escapes, and is carried upwards by the stream of gases, and condenses as a kind of sublimate on the surface of the materials in the upper part of the furnace: considerable quantities of cyanides are also carried up in this way. The alkaline salts thus condensed in the upper portions of the furnace are again brought down to the level of the tuyeres as the materials sink; hence each particle of alkali metal does duty over and over again, the alkalies introduced in small quantities in the fuel, &c., thus accumulating in the furnace to a very large extent. The enormous amounts of upwards of 4 cwt. of alkali metals and 2 cwt. of cyanogen per ton of iron made, have been repeatedly found in the gases near the level of the tuyeres. It appears very probable that the large excess of fuel required during the first few days of starting a new blast furnace is mainly due to the circumstance that cyanides have not formed to any great extent from the necessary want of this accumulation; after a very short time, however, considerable amounts of cyanides are found to be present in the gases.

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* The production of cyanide on the small scale in this way as a lecture illustration appears to be facilitated by the admixture of oxide of iron with the prepared charcoal, but is not always successful, the cause of failure being unknown.

These reactions afford an explanation of a circumstance which has for a long time been a difficulty in the minds of chemists who have studied the blast furnace. All observers agree that when the oxygen and carbon contained in the gases at various levels of the furnace are compared with the nitrogen, there is always more oxygen present than in ordinary air, and always more carbon than could be there were ordinary air burnt wholly to carbon oxide: thus the following Table illustrates the average composition by weight found by Bell in the case of an 80-foot furnace using calcined limestone:—

| Distance above Tuyere in feet. | 0 | 6 | 12 | 25 | 37 | 50 | 60 | 76·5 | Blast if wholly burnt. | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | To CO. | To $CO_2$. |
| Carbon dioxide .. | 1·2 | trace | 0·8 | 1·2 | 1·6 | 1·2 | 3·5 | 7·9 | — | 29·2 |
| Carbon oxide .. | 37·6 | 37·1 | 35·9 | 34·9 | 34·8 | 34·8 | 33·2 | 33·0 | 34·4 | — |
| Nitrogen .. .. | 61·2 | 62·9 | 63·3 | 63·9 | 63·6 | 64·0 | 63·3 | 59·1 | 65·6 | 70·8 |

CARBON and OXYGEN in the GASES at DIFFERENT LEVELS, calculated per 100 of NITROGEN.

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Carbon .. .. | 26·8 | 25·2 | 24·6 | 23·9 | 24·1 | 23·8 | 24·0 | 27·5 | 22·5 | 11·3 |
| Oxygen .. .. | 36·5 | 33·7 | 33·3 | 32·6 | 33·1 | 32·4 | 33·9 | 41·6 | 30·0 | 30·0 |

The presence of this excess of oxygen is readily accounted for when it is remembered that even at the lowest portions of the furnace some oxygen is still left in the ore, which is eliminated only at that point, necessarily in association with carbon, and that at this level the nitrogen of the blast is partially removed in the formation of cyanides. The anomaly, however, presents itself that the amount of oxygen appears to *diminish* from the tuyeres to a point some 10–12 feet above them, whereas it might be expected to increase, since the reduction of iron oxide is going on in the interval between the tuyeres and this point; inasmuch, however, as nitrogen is probably eliminated from the cyanide by its reaction on the residual oxide of iron throughout the whole of this interval, it results that *the nitrogen increases relatively to the carbon and oxygen in the gases*; or, what is the same thing, these diminish in reference to the nitrogen.

We are now in a position to trace out the general chemical changes undergone by the oxide of iron in passing through the furnace; for this purpose we may divide the furnace into three regions; in the uppermost tendencies [A] and [D] jointly are stronger than [B] and [C] jointly, and hence rapid reduction of ferric oxide takes place: in this region, too, tendency [C] is more powerful than [D] and [E] jointly, and hence carbon deposition takes place to a large extent. In this region, too, the limestone is for the most part calcined into quicklime, whilst if raw coal is the fuel employed, it is here coked: if carbonate of iron be used instead of oxide, it becomes converted into oxide in this region, the reducing and carbon-depositing reactions going on simultaneously with the formation of oxide.

In the middle region the iron-reducing tendencies are almost balanced by the iron-oxidizing ones, whilst the carbon-depositing tendencies are equalled and perhaps slightly excelled by the carbon-oxidizing tendencies; here reduction takes place, but only languidly, the chief effect produced in passing through this region being an increase of temperature.

In the lowest region, the reduction of the residual iron oxide is completed chiefly through the agency of the cyanides formed in the vicinity of the tuyere; the reduced iron melts, dissolving a certain amount of the finely-divided carbon in contact with it, together with small quantities of sulphur, silicon, and phosphorus reduced by subsidiary reactions. The earthy constituents of the ore and the lime of the limestone also fuse, forming "slag" as above described. In the act of cooling, this solution of amorphous carbon in molten pig iron undergoes a remarkable change, whereby the carbon is converted to a greater or less extent into the allotropic modification graphite, which is insoluble in molten iron, and so separates in crystals, thereby giving a crystalline structure to the pig, and forming "grey" iron of a quality varying with the nature of the foreign ingredients which retard the allotropic transformation (*specimens exhibited*): thus in white iron and in spiegel-eisen the transformation does not take place to any appreciable extent before solidification. This allotropic change is precisely similar to that undergone by a solution of yellow phosphorus in carbon disulphide by exposure to light; in each case the more stable allotrope formed (graphite and red phosphorus) possesses less "intrinsic chemical energy," *i.e.* gives out less heat on combustion, so that the allotropic change is attended with an evolution of heat. The accompanying specimens of recently-prepared phosphorus solution (perfectly clear), and a portion of the same solution exposed to sunlight for some days (full of flakes of red phosphorus), illustrate this change. (*Specimens exhibited.*)

This action of light on phosphorus is in several respects analogous to the changes produced in various attenuated vapours by passing a powerful beam of light through them, as ably described and illustrated in this room by your Professor of Physics on a former occasion.

Had time permitted, I should have wished to refer to the practical consequences of these researches of Mr. Bell. In order to effect the smelting of iron a certain definite amount of heat is required to perform the general work of the furnace; the various items in this amount are indicated by the following Table:—

### Appropriation of Heat in an 80-foot Furnace, during the Production of 20 cwt. of Pig Iron from Cleveland Ore.

| | Cwt. Heat-units. | |
|---|---|---|
| Constant requirements of furnace— | | |
| Reduction of Fe from $Fe_2O_3$ | 33,108 | |
| Impregnation with carbon | 1,440 | |
| Reduction of P, S, and Si | 4,174 | |
| Fusion of pig iron | 6,600 | |
| Radiation from walls of furnace | 3,658 | |
| Cooling tuyeres by water | 1,818 | |
| Conduction to earth, and other sources of loss not determined | 3,202 | |
| | | 54,000 |
| Variable sources of loss of heat— | | |
| Fusion of slag | 16,720 | |
| Expulsion of $CO_2$ from limestone | 5,054 | |
| Decomposition of ditto | 5,248 | |
| Decomposition of $H_2O$ in blast | 2,720 | |
| Evaporation of $H_2O$ in coke | 313 | |
| | | 30,055 |
| Carried out by escaping gases | | 8,860 |
| Total | | 92,915 |
| Brought in by hot blast | | 11,919 |
| Heat produced by combustion of coke | | 80,996 |

To produce this heat with a minimum expenditure of fuel, it is necessary that the whole of the carbon used as fuel should be oxidized in the furnace to carbon dioxide; the relative strengths of the forces involved in the 9 reactions above described are, however, such that it is not possible to convert more than about 35 or 40 per cent. of the carbon burnt into carbon dioxide, the rest necessarily escaping as carbon oxide. Hence much less heat is generated in the furnace than would be if the fuel could be wholly burnt to carbon dioxide; that is, more fuel must be used to do the work of the furnace. It hence results that if the exigencies of commerce or of nature should require that metallic iron should be obtained from its ores with the consumption of a materially less amount of fuel than is now necessary for the working of a blast furnace of the best description, some wholly dissimilar form of apparatus will be requisite for the purpose.

[C. R. A. W.]

## WEEKLY EVENING MEETING,

Friday, March 20, 1874.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

### William B. Carpenter, M.D. LL.D. F.R.S.

## *On the Temperature of the Atlantic.*

Among the objects of the 'Challenger' Expedition, a careful and detailed examination of the Temperature of the great Ocean-basins was one of the most important, in its bearing alike on the Physical Geography of the Deep Sea, and on that hypothesis of a General Oceanic Circulation sustained by difference of Temperature alone, which had been advocated by Dr. Carpenter on the basis of the previous inquiries in which he had taken part. And having, by the kindness of Admiral Richards, late Hydrographer to the Admiralty, been furnished with the full results of the 'Challenger' Temperature-survey of the North and South Atlantic, now completed, he proposes to compare these results with the predictions he had ventured to put forth on theoretical grounds, and to inquire what modifications may now be called for in the received doctrine of the Gulf Stream.

In his discourse on the results of the 'Lightning' Expedition of 1868, delivered to the members of the Royal Institution on the 9th of April, 1869, Dr. Carpenter referred to the doctrine at that time current among Physical Geographers, of a uniform Deep-Sea temperature of 39° Fahr.; the thermometer being supposed to *rise* to this point with increase of depth in Polar seas, and to *sink* to this point with increase of depth in Equatorial seas. And he showed that this doctrine was inconsistent (1) with the observations recently taken under his own direction in the deep channel between the North of Scotland and the Faroe Islands, (2) with the observations taken not long previously by Captain Shortland, of H.M.S. 'Hydra,' in the Arabian Gulf, and (3) with various observations taken in other localities, in which temperatures *below* 39° had been met with at great depths. He might have added (but was not then aware of the fact) that Captain Spratt had ascertained the temperature of the Mediterranean, at depths corresponding to those of the great Ocean-basins, to be much *above* 39°, ranging as high as 56°. Further, a difference of 14° having shown itself between the bottom-temperatures of adjacent areas at corresponding depths in the 'Lightning' Channel,—the temperature in the "warm area" being as much *above* 39° as that of the "cold area" is *below* it,—Dr. C. had ventured to affirm that these facts "may with confidence be taken as an example of that continual interchange between the Oceanic waters of Equatorial and Polar Regions, which is as much a physical necessity as that interchange of air which has so large a share in the production of winds."

The Temperature-investigations carried on in the 'Porcupine' Expedition of 1869, of which a summary was given by Dr. Carpenter in his discourse of February 11, 1870, furnished a large body of additional facts in support of this view; whilst they also afforded the explanation of the previous misapprehension, this being due to the enormous pressure to which Deep-Sea thermometers are subjected (one ton per square inch for every 800 fathoms of depth), whereby the indications of all such as are unprovided with "protection" against it are vitiated. In the observations of 1869, as in all subsequently taken, only "protected" thermometers have been used; and their indications may be relied on as very close approximations to the truth. The bottom-temperature of the "cold area" between the north of Scotland and the Faroe Islands, which, at from 500 to 600 fathoms, is overflowed by a glacial stream more than 300 fathoms deep, is 29½° Fahr.; while by Temperature-soundings taken near the margin of the great Atlantic basin, which rapidly deepens to the west of Ireland, it was found that the stratum of 39° was entered at about 1000 fathoms, and that from this depth downwards the thermometer sank slowly but progressively to about 36½° at 2435 fathoms. The results of the Temperature-soundings previously taken by Commander Chimmo, R.N., and Lieutenant Johnson, R.N., with unprotected thermometers at various points in the North Atlantic basin, when corrected for pressure according to a scale experimentally determined, were found to correspond very closely with these; and Dr. Carpenter had thus felt himself entitled to state with confidence, that the low Deep-Sea temperatures which had been occasionally met with by previous observers, depended, not (as had been supposed) on *local* currents,—but on the *general* prevalence over the whole North Atlantic sea-bed, of a temperature but a very few degrees above the freezing-point of fresh water. And he further considered it obvious "either that the vast body of water occupying the deeper half of the Atlantic basin has been itself derived from a colder region, or that its temperature has been reduced by the diffusion through it of frigid water from a Polar source,"—the latter supposition being in the speaker's opinion the more probable, on account of the narrowness of the communications between the Arctic and the North Atlantic basins, which must greatly limit the flow of glacial water from the former into the latter. And he further expressed the belief that a much more free interchange of water takes place between the Antarctic and Equatorial areas, than is possible in the Northern hemisphere; so that the temperature of the *South* Atlantic sea-bed will be lower than that of the *North* Atlantic; whilst, further, the cooling effect of the Antarctic under-flow may very probably extend itself to the north of the Equator (as it does in the Arabian Gulf), so as to reduce the bottom-temperature even of *intertropical* seas, *below* that of the North Atlantic. This prediction was signally verified soon afterwards; for the Temperature-soundings taken in the Indian Ocean by Commander Chimmo, with "protected" thermometers, nearly under the Equator, gave 33°·6 at a depth of 2300 fathoms, and 32° at 2656 fathoms.

In the discourse which Dr. Carpenter delivered in the following year (March 10, 1871) on the Scientific Researches he had prosecuted during the preceding summer in the Mediterranean, he was able to adduce, in support of the doctrine he had advocated, another evidentiary fact of no small weight; namely, the remarkable contrast of temperature between the *deep* waters of the Atlantic and Mediterranean basins, which are separated by the "marine watershed" formed by the Strait of Gibraltar; the temperature of their *surface*-waters being nearly the same. For during the *winter* months, the temperature of the western basin of the Mediterranean averages about 54° at the surface, and this temperature is maintained with extraordinary uniformity to the bottom at 1500 or 1600 fathoms; while in the summer months it is only the surface-layer that is affected by solar radiation, the temperature from 50 fathoms downwards remaining constant at 54° throughout the year. On the other hand, in the Atlantic basin under the same parallels, the temperature was found to fall gradually from about 54° at 50 fathoms to 50°·5 at 700 fathoms; then *rapidly* to 46° at 800 fathoms, 42° at 900 fathoms, and 38° at 1000 fathoms; and then again *slowly* to 36°·5 at depths exceeding 2000 fathoms.

Since, then, it was argued, the uniformity of the Mediterranean temperature from above downwards clearly proves that Depth *per se* has no influence in reducing it, no other cause can be assigned for the progressive reduction of *eighteen degrees* in the temperature of the Atlantic between the same parallels of latitude and at corresponding depths, than an under-flow of Polar water. For this movement a sustaining power was found in the constant disturbance of equilibrium maintained by the action of Polar cold; the application of which to the surface must produce a continual *downward* movement, just as the application of heat at the bottom in the hot-water apparatus by which large buildings are warmed produces an *upward* movement; and as the weight (or *downward* pressure) of the Polar column must exceed that of the Equatorial column,—these levels being assumed as equal,—in proportion to the increase of density occasioned by reduction of temperature, the excess of *lateral* pressure will occasion an outflow of bottom-water from the Polar area towards the Equatorial; and this, by reducing the level, will occasion an indraught of surface-water from the Circum-polar area, which must in its turn draw upon the Temperate, and the latter upon the Intertropical.

No Physicist can doubt the theoretical truth of this position, which was experimentally illustrated on two former occasions; the only question is, whether there is adequate evidence that such a double movement really exists.

Previously to the departure of the 'Challenger' Expedition, Dr. Carpenter had obtained the following additional evidentiary facts, which afforded a strong confirmation of his views:—

1. The Sulu Sea, which lies between the N.E. of Borneo, and Mindinao (the southernmost of the Philippine group), bears somewhat the same relation to the China Sea that the Mediterranean does to the Atlantic. For, although not enclosed by continuous land, it is shut in at no great depth by submarine reefs, within which there is depth of no less than 1800 fathoms. Now Commander Chimmo, who has lately surveyed this area, found that while the surface-temperatures of the Sulu and the China Seas were similar, and the rate of descent of the thermometer through the superheated stratum was nearly the same in both, the temperature of the China Sea falls to 51° at 200 fathoms, and to 37° from 550 to 900 fathoms; but that the temperature of the Sulu Sea falls so much more slowly that it does not reach 50° until 1100 fathoms, and then remains constant to the bottom at 1778 fathoms. It does not seem possible to account for this fact on any other hypothesis, than that the temperature of the deeper stratum of the China Sea is lowered by a flow of cold water from the *Antarctic* area (the narrowness and shallowness of Behring's Strait entirely excluding *Arctic* water); whilst the temperature of the deeper portion of the Sulu Sea, which is shut in by a wall that may be likened to an enormous crater, is that of the coldest stratum that can find admission into it.

2. The investigations recently made by Captain Wharton into the currents of the Black Sea straits, have shown that a very powerful under-current may be produced by a slight difference of downward pressure. Owing to the excess of its rain and river supply above the amount dissipated by evaporation, there is at most periods of the year a surface-outflow from the Black Sea, through the Bosphorus and the Dardanelles, into the Ægean. The water of the Black Sea has something less than half the salinity of that of the Ægean; so that supposing the heights of the two columns to be equal, there would be an excess of lateral pressure in the bottom-water of the Ægean, tending to produce an under-flow backwards into the Black Sea. But a very slight excess of level in the Black Sea column, maintained by the excess of fresh water poured into it, would suffice to equalize the *bottom* and therefore the *lateral* pressures of the two columns; and thus it happens that there is no inward under-current, except when the outflow is aided by a wind which sets down the Straits during a large proportion of the year. When this wind blows strongly, so as to keep down the excess of water in the Black Sea, and raise the level of the Ægean (however slightly), the excess of density in the Ægean column exerts its effect; producing an inward under-current of such strength, that the current-drag suspended in it dragged the suspend- buoy in opposition to the surface-current, at such a rate that only am-launch of the 'Shearwater' could keep up with it.

3. A similar under-flow produced by difference of density has been noticed at the mouth of the Hudson river, which discharges itself through the "Narrows" into New York harbour. During a large part of the year, the excess of fresh water brought down by the river keeps up its level to a degree sufficient to neutralize the excess of density of the water outside the Narrows, and there is no influx of salt water. But in the summer, when the river is at its lowest, an inward under-flow of sea-water is traceable for a considerable distance up its channel; this being produced solely by the excess of lateral pressure.

4. The Temperature-observations taken in the New York School-ship 'Mercury' on her voyage from Sierra Leone to Barbadoes (kindly communicated to the speaker by Professor Draper), brought to light this remarkable fact in regard to the temperature of the Intertropical portion of the Atlantic,—that *cold water lies nearer to the surface* in this area, than it does in any part of the temperate North Atlantic. And, as the speaker had pointed out ('Proceedings of the Royal Society,' No. 138, p. 541), "this fact, if confirmed by further inquiries, will furnish a powerful argument in support of the doctrine of a *vertical* Oceanic Circulation; since it is obvious that as it will be in the Intertropical area that the cold bottom-water will be most rapidly brought to the surface, the temperature of the stratum not yet within reach of direct insolation may be expected to be lower than it is in the Temperate area."

Such having been Dr. Carpenter's anticipations, the correctness of the theory on which they were based may now be tested by their conformity to the large mass of Temperature-observations taken in the exploratory Voyage of the 'Challenger' in the North and South Atlantic, during the year 1873.

The first Section was taken obliquely across the North Atlantic, from Teneriffe (lat. 28° N.) to St. Thomas's (lat. 18° N.) between the 14th February and the 16th March. The depth progressively increased, with one remarkable exception, from 1890 fathoms at a short distance from Teneriffe, to 3150 fathoms at long. 35° W., which was the greatest depth encountered in the Eastern basin. That exception was a rather sudden rise from 2000 to 1525 fathoms, at a distance of 160 miles S.W. of Ferro; and Captain Nares remarks,—"The rocky nature of the bottom, and the lowering of the temperature usual at that depth, would indicate a considerable movement of the lower stratum of water." From long. 35° W. to long. 44½° W., the depth gradually diminished to 1900 fathoms; this submarine elevation, called the "Dolphin Rise," divides the Mid-Atlantic into an Eastern and a Western basin. The depth then gradually increased to 3025 fathoms, in long. 61½° W., very near the northern extremity of the chain of the Lesser Antilles or Windward Islands; and the water then rapidly shallowed as Sombrero Island was approached.

The Temperature-section, constructed from serial soundings usually taken at intervals of 100 fathoms down to 1500, and then at longer intervals to the bottom, shows an exact conformity at its western end to the observations previously taken by Dr. Carpenter off the coast of Portugal, in regard to the position of the line of 39°, which was reached at 1000 fathoms; and in regard also to the progressive reduction below this to a bottom-temperature of 35½°. But the temperature of 50° was encountered at a much smaller depth,—about 350 fathoms, instead of 700; so that the transition from 50° to 40° was much less abrupt than it was farther north. The uniformity of the depth, 380 fathoms, at which the temperature of 49° was reached, is a very remarkable feature in this section; especially when we bear in mind that its western extremity is ten degrees farther south than its eastern, and also that the spring was advancing. Thus, although the *surface*-temperature rose from 65° to 76°, the effect of this elevation was quite lost at a depth of 400 fathoms. Below this depth, a remarkable change shows itself, in the gradual upward slope of the isotherms of 45°, 40°, and 39°; so that the latter temperature, which was entered at the eastern end at 1000 fathoms, is encountered over the deepest part of the western basin at 850 fathoms, and the strata of 50°–45° and 45°–40° are by so much thinner. But the most significant phenomenon is the depression of the *bottom*-temperature from its remarkably uniform level of 35°·5 in the deepest portion of the eastern basin, to 34°·4 in the deepest portion of the western. This depression, taken in connection with other facts to be presently adduced, clearly indicates that an under-flow of *Antarctic* water extends as far north as St. Thomas's.

The 'Challenger' left St. Thomas's on the 24th March, and proceeded due north to Bermuda (lat. 32° N.). At a short distance to the north of St. Thomas's, an extraordinary depression of the bottom, to the depth of 3875 fathoms, was encountered. The details given by Captain Nares of this remarkable sounding—the deepest yet obtained on which reliance can be placed—leave no reasonable doubt of its trustworthiness; more especially when taken in connection with this significant fact, that the two "protected" Thermometers which had previously stood the pressure of nearly *four tons* on the square inch, were both crushed by the pressure of 4¾ tons which they here encountered, thus preventing the determination of the bottom-temperature. Past this depression, the bottom rose again to between 2800 and 2900 fathoms; and thence gradually to 2475 fathoms in the immediate neighbourhood of the Bermuda group, which seem to rest on a column nearly three miles high, that rises from a very small base. There is strong reason to regard this column as a Coral-formation, commenced when the present deep bottom was near the surface, and kept up to it by progressive growth as the bottom gradually subsided.

The most noticeable feature in the Temperature-section between St. Thomas's and Bermuda, is the great increase in the thickness of the stratum between 60° and 65°; this increase being produced, not only by the reduction of the surface-temperature, and the thinning-out of the superheated stratum, consequent upon the northward direction of the section, but by the lowering of the isotherm of 60° from 200 to 300 fathoms, though the isotherm of 40° rather rises than falls.

The meaning of this peculiarity, which was again encountered on the voyage from Bermuda to the Azores, will be presently inquired into.

Leaving Bermuda on the 21st April, the 'Challenger' proceeded towards New York, crossing the Gulf Stream, and carrying a line of Temperature-soundings to the edge of the shallow water off Sandy Hook. Immediately to the North of Bermuda, a depth of 2650 fathoms was met with; and this increased to 2850, still exceeding 2600 beneath the Gulf Stream, and then rapidly diminishing towards the coast of the United States. Passing on thence to Halifax (Nova Scotia), the 'Challenger' arrived there on the 8th May; leaving it again on the 19th for Bermuda, where she arrived on the 31st, after again crossing the Gulf Stream, and examining the singular temperature-phenomena of the cold band which separates it from the coast of America.

In both these Sections, the condition of the *deeper* stratum corresponds closely with that previously described. From Bermuda northwards to the Gulf Stream, the isotherm of 40° lies at a depth of about 650 fathoms; and below this to the bottom at from 2500 to 2800 fathoms, there is a progressive descent of the thermometer to a little below 35°, so that the thickness of the deep stratum having a temperature of from 35° to 40° averages above 2000 fathoms, or 12,000 feet. The *upper* strata here present some very singular features. The four successive layers, 40°–45°, 45°–50°, 50°–55°, and 55°–60°, are here comparatively thin; so that a fall of no less than *twenty degrees* shows itself between about 330 and 620 fathoms. On the other hand, the layer whose temperature exceeds 60° has a greatly augmented thickness; the isotherm of 60°, which lay at 200 fathoms at St. Thomas's, here lying at about 330; notwithstanding a considerable reduction in the temperature of the *superficial* stratum, which makes the passage across the warm current of the Gulf Stream the more obvious. It is perfectly clear from both the New York and the Halifax sections, that the *true* Gulf Stream or Florida Current is a limited river of superheated water; of which the breadth is about 60 miles near Sandy Hook, whilst near Halifax it has separated into divergent streams forming a sort of delta; its depth (as determined by the use of the current-drag) being nowhere more than 100 fathoms. This river rests upon the remarkable stratum of 60°–65°; the thickness of which, as we shall presently see, distinguishes the Western from the Eastern Atlantic between Bermuda and the Azores; while at less than double the depth of that layer, we come into what is clearly Polar water.

Now it has been long known that the Gulf Stream is separated from the United States coast by a band of water, of which the temperature is as much *below* the normal of the latitude, as that of the Gulf Stream is *above* it; the passage from one to the other being so abrupt, as to have been termed the "cold wall." And this band has been regarded as the continuation of the "Greenland and Labrador Current"; which, propelled by northerly winds, runs past Newfoundland, and then, turning the corner of Nova Scotia, passes across to

Cape Cod, "hugs the shore" along the whole U.S. Atlantic seaboard, and extends even as far south as the Florida Channel itself. But of this southward extension of the Labrador Current there is no evidence whatever beyond that of *temperature;* no *surface-movement* being traceable in this cold band to the south of New York; and its existence has obviously been a perplexity to the U.S. Coast Surveyors, who traced its continuity with the cold stratum lying beneath the Gulf Stream. This continuity comes out most remarkably in the 'Challenger' Temperature-section between the Gulf Stream and Halifax; for we there see, not only (as in the New York section) the bathymetrical isotherms of 60°, 55°, and 50°, but the yet deeper isotherms of 45° and 40°, successively rising to the surface as we approach the land; while at a depth of only 83 fathoms, a temperature of 35° is encountered, which, at no great distance to the south, would only be found at a depth of 2000 fathoms.—The *rationale* of this remarkable surging-upwards of the cold deep strata along the coast-line will be considered hereafter.

Leaving Bermuda June 12th, the 'Challenger' again crossed the Atlantic in an easterly direction, first passing slightly northwards (from lat. 32° N. to 37½° N.) to the Azores, and then slightly southwards (from 37½° N. to 33° N.) to Madeira, where she arrived July 16th. The greatest depth met with between Bermuda and the base of the gradual slope which culminates in the Azores, is 2875 fathoms; and the lowest bottom-temperature is 35°. The most noticeable feature in this Section is the extension of the thick layer of 60°–65° as far eastwards as long. 41° W.; and then its rapid thinning, by the approximation of the isotherm of 60° to the surface. At the same time the isotherm of 40° gradually deepens; and the four intermediate bands resume nearly the same proportions that they present in the eastern part of the section from Teneriffe to St. Thomas's. Thus it is obvious that there is a much larger quantity of heat in the upper 300 fathoms of the *western* half of the Atlantic, between about lat. 25° N. and lat. 40° N., than there is in the *eastern;* and this is attributable to the reflux of that portion of the great Equatorial Current which never enters the Caribbean Sea or the Gulf of Mexico, but which, striking against the line of the West India Islands, the peninsula of Florida, and the coast of Georgia, is first deflected northwards, and then turns eastwards towards the Azores and the coast of Africa,—thus completing that *horizontal* circulation in the North Atlantic which is initiated by the Trade Winds. It is quite a misnomer to speak of this as "a branch of the Gulf Stream," since there is no evidence that it either enters or comes out from the Gulf of Mexico.

The next series of Temperature-observations was taken in nearly a north and south direction along the eastern border of the Atlantic, from Madeira to the Cape de Verde Islands, and thence to a position in lat. 3° N. and 15° W. The most noticeable feature in this Section was the progressive diminution in the thickness of the stratum above 40°, notwithstanding a progressive increase in the surface-temperature from 71° to 79°, consequent upon the approach to the Equator. Thus the isotherm of 40°, which at Madeira lies at about 900 fathoms depth, and which half-way towards St. Vincent is about 950 fathoms, rises to 650 fathoms at St. Vincent, and at the Equatorial position actually lies at a depth of no more than 300 fathoms, below which, down to 2500 fathoms, the whole under-stratum has a temperature that falls very gradually from 40° to 35°.

This thinning-out of the upper and warmer stratum in the Equatorial zone is still more marked in the next Section; which was carried obliquely across the Equator from the last-named position to St. Paul's Rocks, which lie almost on the Equator in long. 30° W., thence to the island of Fernando Noronha, about 4° south of the Line, and in long. 32½° W., and thence to Pernambuco in lat. 7½° S. Throughout this Section, the thinness of the stratum above the isotherm of 40° is in most striking contrast with its thickness in the section from Teneriffe to St. Thomas's; for this isotherm, between Fernando Noronha and Pernambuco, actually rises to within 300 fathoms of the surface. There is here a division between the Eastern and Western basins, formed by a ridge that seems continuous with the "Dolphin Rise"; the bottom-temperature of the eastern basin, which has an average depth of 2500 fathoms, very closely approximates to 35°; but in the western basin a temperature of 35° is encountered at about 1800 fathoms, and there is a further progressive reduction to 33°·2 at 2275 fathoms, and 32°·4 at 2475 fathoms. Thus the bottom is here overlaid by a stratum of 600 fathoms' thickness, which is so much colder than the bottom-stratum of any but the Intertropical portion of the North Atlantic, as to be clearly derived from an *Antarctic* source; and there is nowhere a greater contrast, not only between the heat of the surface and the cold of the bottom, but between the thinness of the warm surface-film and the enormous thickness of the almost ice-cold body of water it covers, than in this Equatorial section. For from 78° at the surface, the thermometer falls to 55° within 100 fathoms, just as it does in the Mediterranean; but whilst in that Inland Sea the temperature remains constant from that point to the bottom, it goes on falling under the Equatorial sun to 45° at about 220 fathoms, to 40° at 300, and thence progressively to 32°·4 through a stratum of more than 2000 fathoms thickness.

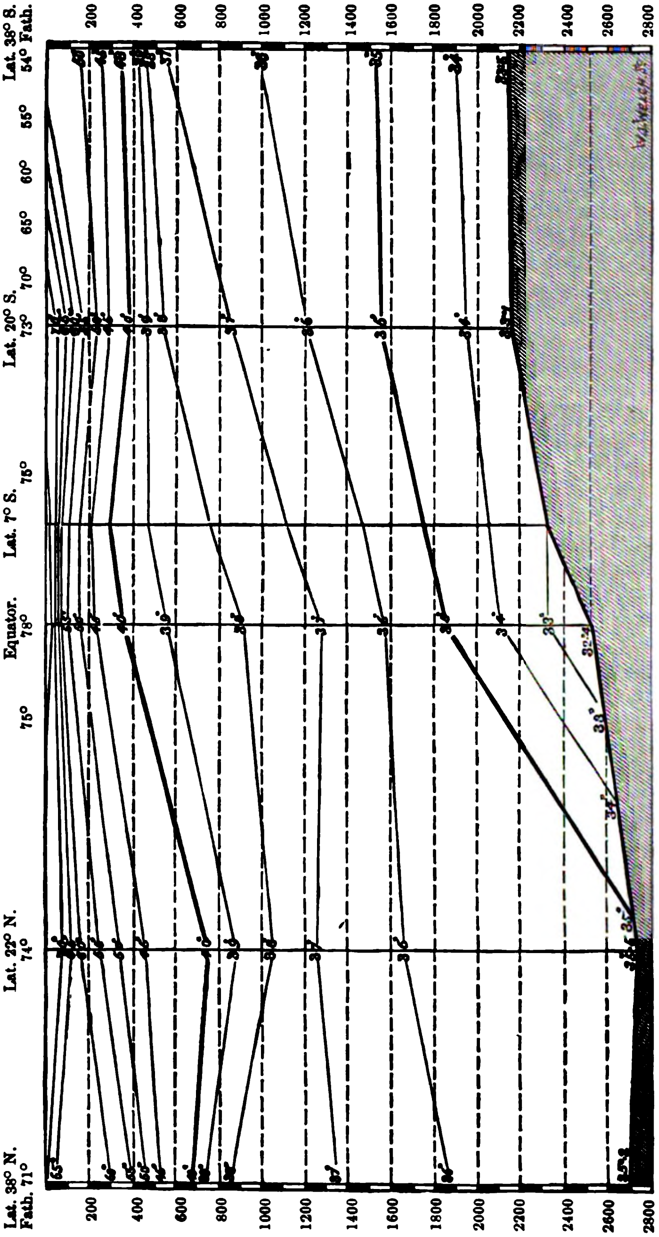
Another very important feature in the Physical condition of this Equatorial water, on which Dr. Carpenter had laid stress in a former discourse (March 10, 1871, p. 21), was brought out very distinctly by the 'Challenger' observations; *viz.* the reduction in the Salinity of the surface-water, as indicated by its Specific Gravity, so that it came to correspond more closely with that of the bottom-water, which, in the tropical and extra-tropical portion of the North Atlantic it considerably exceeds. Thus the mean of eight observations between St. Thomas's and Bermuda gave 1·0272 as the Specific Gravity of surface-water, and 1·0263 as that of bottom-water; whilst the mean of seventeen observations on surface-water between the Cape de Verde Islands and Bahia gave a Specific Gravity of 1·0263, and a mean of eight observations of bottom-water a Specific Gravity of 1·0261.

This, like the diminished thickness of the warm upper stratum, affords a very striking indication of the ascent of bottom-water towards the surface, which, on the theory of the Vertical Circulation, will take place in the Equatorial region, where the two Polar under-flows meet, while the warm upper layer is being constantly draughted off in both directions.

Quitting Bahia September 25th, the 'Challenger' proceeded along the coast of South America to Abrolhos Island, in lat. 20° S.; thence obliquely across the South Atlantic, to Tristan d'Acunha in lat. 36° S.; and thence nearly in the same parallel to the Cape of Good Hope, which was reached at the end of October. This section shows a well-marked division of the South Atlantic also into two basins; the island of Tristan d'Acunha being the culmination of a ridge which very probably extends northwards to the Dolphin Rise. The greatest depth met with in the western basin was 2350 fathoms, and the lowest bottom-temperature 33°·1; the greatest depth met with in the western basin was 2650 fathoms, and the lowest temperature 32°·9. It might have been expected that the bottom-temperatures in the western basin would have been *lower* instead of higher than those taken in the Equatorial section; since the glacial water of the latter must have reached it through some deep channel, probably not far from the coast of South America. The absence of any such indication, however, is readily accounted for by the fact, that circumstances prevented the Temperature-soundings in this basin from being taken at near intervals, some of them being as much as 600 miles apart; so that the required channel may well have lain between them. The existence of a colder under-flow than that which reduces the temperature of the deepest parts of the North Atlantic basin, is evidenced by the fact that the isotherm of 35° sometimes rises to 1500 fathoms; so that a stratum of water between 35° and 33° covers the sea-bed of the South Atlantic to the depth of 1000 fathoms or more.

The upper portion of this Section is of much interest, as compared with that of the Equatorial section on the one hand, and with that of the North Atlantic sections on the other. As the distance from the Equator increased, the surface-temperature rapidly diminished, although the summer of the southern hemisphere was approaching; so that at Tristan d'Acunha the surface-temperature was only 52°. Its descent from this point to 40° was not rapid and was nearly uniform; the isotherm of 40° now again receding from the surface, and lying at a depth of from 400 to 500 fathoms. The excess of *thickness* of this upper warm stratum over that of the Equatorial zone, whilst the quantity of heat it contains is so much less, is a noteworthy fact; on the other hand, the inferiority of this stratum, alike in thickness and in the quantity of heat it contains, to that of the North Atlantic section at about the same distance from the Equator, is still more remarkable.

All these points are strikingly brought out in the accompanying Section, constructed by Dr. Carpenter from soundings selected in such a manner as to combine in a nearly North and South direction.



North and South Section of the Atlantic Ocean, between 38° N. Lat. and 38° S. Lat.

Passing on now to inquire into the *rationale* of these phenomena, we seem justified in assuming it as a principle based alike on theory and on observation,—that any water which is *colder* than the Isocheimal (or lowest mean winter temperature) of the Latitude, must have come from a source more distant from the Equator; and that if such water has a *glacial* temperature, it must have come all the way from one of the Polar areas. For suppose an area of (say) 100 miles in diameter to be enclosed in any part of the Atlantic by a circle of reefs rising up from the bottom to within 50 fathoms of the surface, we are fully justified by the thermal condition of the Mediterranean, of the Sulu Sea, and of the Gulf of Suez (which was found by Captain Nares to have the uniform temperature of 71° from its surface to its bottom at 450 fathoms in the month of February), in affirming that the temperature of this area would in winter be uniform from the surface to the bottom, and that in summer only the superficial stratum would be superheated.

Now if such an area were so enclosed between St. Paul's Rocks and Fernando Noronha, its temperature would be certainly not less than 70°, and probably as high as 75°, from the surface to the bottom; but as we find that even at 100 fathoms it is at least 15° lower, at 240 fathoms 25° lower, at 320 fathoms 35° lower, and from 320 fathoms to the bottom at 2475 fathoms nearly 40° lower, we seem justified in saying with certainty (1) that almost the entire mass of water from 300 fathoms downwards must have come thither from a Polar source, and (2) that even the superficial stratum between 300 and 100 fathoms has had its temperature greatly reduced by admixture with Polar water.

Supposing, again, such an area to be secluded from the South Atlantic basin in the neighbourhood of Tristan d'Acunha, its uniform temperature from a small depth below the surface would be about 50°; but since from 350 fathoms downwards the temperature is from 10° to 17° below this, we are fully justified in asserting that the subjacent stratum of more than 2000 fathoms thickness must have come from a source much farther distant from the Equator.

If, lastly, our secluded area were midway between Bermuda and the Azores, we might expect to find its uniform temperature of about 60°; but on looking at the actual temperature, we find that, after passing through the superheated stratum, we first come into one *above* 60°, which extends downwards to more than 300 fathoms depth; and that the reduction to a temperature *below* the normal, which indicates an admixture of colder water, only shows itself below that depth. Here we may infer that the *upper* stratum has come thither from a *warmer* locality, and that the temperature of the *lower* stratum has been reduced by water coming from a *colder*.

If, on the other hand, an area were similarly secluded in the neighbourhood of the Faroe Islands, its uniform temperature would be certainly below 40°; but there we find, at the edge of the Atlantic basin, a stratum whose temperature ranges from 48° to 40°, extending downwards to a depth of at least 700 fathoms; whilst the lower half of the channel which lies between the Faroe Banks and the Shetland Islands, is occupied by a glacial stream whose temperature ranges from 32° downwards to 29°·5. Now it is here just as obvious that the deep stratum which is *warmer* than the normal comes from a *southern* source, as that the deep stratum which is *colder* than the normal comes from a *northern* source. And by combining into a Section the 'Porcupine' Temperature-soundings taken between the Faroe Islands and the coast of Portugal, Dr. C. made it obvious that there is a continuity of the whole upper stratum between these two points, down to at least 700 fathoms, with very little loss of heat except in the superficial layer. And this continuity, by which a body of water that is *below* the normal off the coast of Portugal, comes to be *above* the normal off the Faroe Islands, cannot be accounted for in any other way than by a *slow northerly movement* of that water extending down to at least 700 fathoms.

Now it seems obvious that since the *true* Gulf Stream or Florida Current has not, even when running strongly past Sandy Hook, a depth exceeding 100 fathoms,—and since that depth gradually diminishes as it spreads itself out like a fan after passing Halifax, so that it ceases to be distinguishable in the Mid-Atlantic either by its motion or by its temperature,—there is no valid ground for attributing to it that northward movement of the whole upper stratum of the Atlantic, which is shown by the course of the isothermal lines to range from Newfoundland to the west coast of Ireland, and by the 'Porcupine' Temperature-soundings to extend to a depth of at least 700 fathoms. On the other hand, this northward movement is in *exact accordance* with the doctrine of a General Oceanic Circulation sustained by Thermal opposition; the *northward* movement of the *upper* stratum being the necessary complement of that *southward* movement of the *lower* stratum, of which the 'Challenger' survey of the Atlantic affords such cogent evidence. Now in so far as the temperature of the upper stratum is elevated in the parallel between 30° and 40° N. lat., by that reflux of the Equatorial current which seems there to produce the remarkable thickening of the stratum between 65° and 60° already referred to (pp. 268-270), the heating power of that upper stratum, when it is drawn into a higher latitude, is augmented; and to that extent the amelioration of the climate of the British Isles, the coast of Norway, &c., while directly depending on the *vertical* Oceanic Circulation, may be considered as the greater for this *horizontal* transport of Equatorial water by the Trade-winds.

That the deeper stratum of Atlantic water has *a definite movement*, however slow, towards the Equator, is indicated by several facts. Thus it is well known that icebergs occasionally cross the Gulf Stream off the Banks of Newfoundland, and are carried to the south of it; and this can only be through the southerly movement of the deeper stratum in which the lower part of the mass is immersed, which carries it along against the counter-action of the upper current,—just as in the Black

Sea straits the action of the under-current upon the current-drag suspended in it, drags inwards the suspending-buoy, in opposition to the powerful outward surface-current in which it floats. And so the buoy which was attached to the broken end of the Atlantic Cable of 1865, having got adrift, was found to have travelled nearly due south, a distance of 600 nautical miles in seventy-six days, in opposition to the Gulf Stream; presumably by the action of the under-flow upon the long buoy-rope suspended in it.*

But the 'Challenger' Temperature-sections afford another body of evidence to the same effect, which is not a little remarkable. For, as already pointed out (p. 270), they prove that continuity of the cold band which separates the Gulf Stream from the American seaboard, with the deep cold strata underlying the Gulf Stream, which had been previously indicated by the U.S. Coast Surveyors, who had traced it out in the Florida Channel (where there is clear evidence of an inward under-current of glacial water), and to a certain depth also in other parts of its course. This cold band is clearly produced, therefore, by the surging-upwards of the deeper stratum of Atlantic water along the western slope of its basin. And an adequate *vera causa* for this surging-upwards is found in the Earth's rotation, if this stratum has a movement of its own from the Pole towards the Equator. For just as the Gulf Stream, and the whole *north*-moving warm *upper*-stratum, constantly tend towards the *east*, in virtue of the *excess* of easterly momentum which they bring with them from a portion of the globe whose rotatory movement is *more* rapid, so the cold *under*-stratum, if moving southwards from a portion of the globe whose rotatory motion is *less* rapid, will bring with it a *deficiency* of easterly momentum, or, in other words, will tend towards the *west*.

This is by no means a solitary case. Dr. Carpenter is informed by Captain St. John, who has been engaged for some years in the survey of the Japan Sea, that a similar cold band separates the Kuro Siwo (or warm Japan Current, sometimes called the Gulf Stream of the Pacific) from the eastern coast of Japan. And Dr. Meyer of Kiel, who has been for some time engaged in the examination of the physical condition of the Baltic, the North Sea, and their connecting channels, has communicated to him this remarkable fact:—While the greater part of the North Sea forms part of the platform of less than 100 fathoms depth, which surrounds the British Islands, and which serves (as formerly pointed out by Dr. C.) as a coast-line to the glacial water lying at its northern edge at a depth of over 200 fathoms, there is a channel along the coast of Norway and Sweden, which is deep enough to receive a cold stream from the Polar Sea; and this may be traced as far south as the Skager Rack. Now this cold stream surges-up on the western bank of its channel, and overflows the bed of the North Sea, as far as the Dogger Bank; the temperature of whose eastern slope is thus reduced by at least *ten degrees* below that of the western, the difference being observable within very short distances.

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* In two Papers recently written by Mr. Croll on the Physical Cause of Ocean Currents ('Philosophical Magazine,' February and March), it is maintained not only that the true Gulf Stream is propelled onwards by a *vis a tergo* into the Arctic area, but that, being there forced into a closed basin, it produces the under-flow of Polar water by forcing out an equivalent amount of the deeper stratum. Unfortunately for this hypothesis, however, a very simple calculation shows that the sectional area of the Polar under-flow in the parallel of New York is at least *nine hundred times* that of the Gulf Stream off Sandy Hook.

Since, therefore, so many facts, otherwise quite anomalous, are found to be in such exact accordance with the general Doctrine, as to be predicable by an application of it to each particular case, and since the Doctrine itself has been pronounced by the greatest masters of Physical Theory to be in strict accordance with it,—Dr. Carpenter submitted that he has established a strong claim for its provisional acceptance. And he anticipated that this claim will be greatly strengthened by the 'Challenger' Temperature-section now taken between the Cape of Good Hope and Melbourne, together with the north and south section between Kerguelen's Land and the Antarctic ice-barrier, which last had been included in her instructions at his special instance, as likely to afford the best test of its validity.

[W. B. C.]

# Royal Institution of Great Britain.

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## WEEKLY EVENING MEETING,

Friday, March 27, 1874.

WARREN DE LA RUE, Esq. D.C.L. F.R.S. Vice-President,
in the Chair.

### PROF. A. C. RAMSAY, LL.D. V.P.R.S.

DIRECTOR-GENERAL OF THE GEOLOGICAL SURVEY OF THE UNITED KINGDOM, ETC.

## *On the Physical History of the Rhine.*

THE attempt to unravel geological history, as far as the stratified rocks are concerned, and all the igneous rocks connected with them, simply resolves itself into this:—an effort to realize the physical geography of different geological epochs, to make out the relations of the sea and of the land, with all its plains and mountains during these periods, the history of the rivers and lakes of the time, and to know as much as may be known of all the creatures and of all the vegetation which inhabited the water and the land.

I am now going to attempt to explain a special history, that of the Rhine. Every river has a definite history, if we could clearly make it out. Every river has had a beginning, and it is quite possible —if we have the skill—to find out when, by special changes in physical geography, such and such a river began to flow, and why it flows in such and such a direction.

In various publications I have attempted to show what is the history of some of the rivers of England; as, for example, those of the Severn and of the Thames, and I think I have been able to prove that the Severn is a much older river than the Thames; and, on similar principles, I now propose to attempt to reveal to you the history of the Rhine and its valley from early times to the present day. For many years I had an ambition to work out this problem. I have known it for more than twenty years; going often up and down the river, and sometimes for weeks—once for months—living on its banks. For the last thirteen years, unfortunately, I never was able to return to it, but the question I had marked out for myself remained in my mind, and last year I went, and worked it out, with the result which is now to be explained.

First, with regard to the great main features of the Rhine valley; it has its sources, as every one knows, in the mountain regions of

Switzerland, one of which is in the valley of the Vorder Rhine, and the other in that of the Hinter Rhine, both glacier regions. The ground where it rises is from 7000 to 8000 feet above the level of the sea. From thence it passes to the Lake of Constance, 1305 feet above the sea; and beyond, in a westerly direction, by Schaffhausen to Basel, where, at the bridge, the level of the water has an average height of about 803 feet above the sea. Thence it flows down the great upper plain of the Rhine northerly between the Schwartzwald and the Vosges to Mainz, where the height of the river is 531 feet above the level of the sea, thus showing a fall between Basel and Mainz of 272 feet, which gives an average slope for the fall of the river of about 3 feet 1½ inch per mile. Beyond that, proceeding to the north, we come to the deep gorge of the Rhine, passing between high cliffy banks, which begin at Bingen and continue down to Rheineck in the neighbourhood of the Siebengebirge, for a distance of from sixty to seventy miles, according as you take into account all the windings, or omit them. Beyond the Siebengebirge there is a plain, partly formed of the delta of the river, which gradually merges into the great flats that extend all the way from Calais to the Elbe.

Now the main question I have to bring before you is, first, what is the origin of the great upper plain that lies between Basel and Mainz? and, secondly, what is the origin of the gorge between Mainz and Rheineck? Why are they there, and by what means have this plain and this gorge assumed their present forms?

When you stand above Bingen, or, better still, if you ascend the Taunus and look southerly, and consider the narrowness of the gorge and the hilly barrier of rock that must once have continuously extended at Bingen across the lower end of the plain, the impression is irresistibly conveyed to the mind that before that gorge was opened a vast lake must have reached all the way from that barrier to where Basel now stands, covering the great plain that lies between the mountains of the Vosges and those of the Schwartzwald. And so thoroughly has this idea taken possession of the popular mind, at least of those who have at all considered the subject, that we find this statement made in some of the Guide Books of the time, and notably by Baedeker, where it is stated that a lake must have covered the whole of that vast plain, 170 miles in length, at a comparatively recent period. It is a very obvious theory, and at first sight has much to recommend it; for it seems so clear that, before the gorge was opened, all that plain must have been covered with a sheet of water, and it is hard to realize that such has not been the fact. When I first entered on the subject I was impressed with this idea, and I began to cast about and endeavour to find a reason for the scooping out of the gorge, and for the consequent drainage of the supposed lake.

Having years before written a paper on the origin of the lake basins of Switzerland, North America, and other parts of the world, and having attributed the formation of many of these, but by no means all of them, to the action of glacier-ice during the glacial period, my first impression was that ice might have had at least something to do with the scooping out of the great valley that lies between the north flanks of the Jura, the Schwartzwald, the Vosges, and the Taunus. But while slowly passing up the river, and searching for proofs which might either confirm or contradict this view, I was soon obliged to give up the idea that glacier-ice had anything to do with scooping out the great hollow. For on one side—that of the mountains of the Schwartzwald—I found that none of the glaciers of that region (and there are proofs that glaciers once existed there) even extended well down into the valley of the Rhine. And on the opposite side of the Rhine Valley, that of the old glacier region of the Vosges, I found no proof that they ever extended into the plain. There is also no proof that the glaciers of the great glacial epoch of Switzerland ever spread as far north as Basel. Neither are there any signs of erratic blocks or other kinds of moraine matter on the plains or hill-slopes about Bingen, which we might expect to find there had the whole of the great plain of the Upper Rhine been once filled with glacier-ice. Therefore this theory, which I had not definitely formed, but which I surmised might possibly have had something to do with the subject, entirely melted away, and other hypothetical views along with them, and I was obliged to begin anew.

Accordingly I went to Switzerland, and with the help of friendly Swiss geologists, examined part of the Miocene or Middle Tertiary rocks between the Oberland and the Jura.

To make the rest of the subject clear, I must now say a few words about the origin of mountain chains. Most people are familiar with the outlines of the nebular hypothesis. The whole solar system was once in a nebulous state, and as this nebulous mass revolved in space, portions of it were thrown off, and one of these consisted of the matter which, by and by, resolved itself into the present earth. This nebulous fluid, in virtue of gravity, by degrees condensed more and more, and, passing through what we may call the molten state, in the course of time began to assume a solid form, and a hard outer crust was at length produced, which enclosed a highly-heated fluid mass within. This crust, which continued to thicken in consequence of radiation of heat, because of the law of gravitation was ever drawn towards the centre of the earth. By this process the circumference of the earth necessarily became less, and that consolidated rocky sphere which formed the outer shell of the earth was forced to readjust itself so as to occupy a diminishing area. Thus it happened that while some parts sank, other parts of the crust were crumpled, and relatively raised higher than other portions of the crust that still retained their original curves as part of a sphere.

This hypothesis, which, as far as I know, was first propounded by Elie de Beaumont, may be looked upon as the origin of mountain chains. What began in the earlier times of geological history, seems to have been going on steadily down to the present day, and thus it happens, that geologists can prove mountain chains to be of very different ages, and that, of whatever age they may chance to be, the strata that compose them are found to be bent and contorted.

In this way it happened that at a certain period of geological history which preceded the formation of the Miocene rocks north and south of the Alps, a disturbance of the earth's crust took place, due to shrinkage of the general mass, of such a nature that the Alpine strata were thrown into highly-contorted forms, and a great mountain range of pre-Miocene age was the result. On the north of these mountains the Miocene strata began to accumulate in great lakes. But these lakes lay so near the level of the sea, that every now and then, by depression of the land, they sometimes sank a little, and the sea invaded the area formerly occupied by fresh water. The result was that in Switzerland, between the Oberland and the Jura, and much farther north, the Miocene strata, which are hundreds and sometimes thousands of feet thick, are now found to consist of interstratifications of marine, brackish, and fresh-water beds. At that time the Jura had no existence. It is the result of a later disturbance of the crust of the earth; and thus it happened that the Miocene waters in which were deposited the strata that now lie between the Oberland and the Jura originally spread northwards far across the area now occupied by that range, and into the district of the present plain of the Rhine between Basel and Bingen.

It is hard to realize the scenery of that time; but partly by an effort of imagination, and partly by special knowledge of the fossils contained in the rocks, it is possible to form some conception of the appearance of the country.

On the east and west of the great valley were mountainous ranges now called the Schwartzwald and the Vosges, while far to the south rose the high mountains of the pre-Miocene Alps, more or less covered with a forest vegetation. On the banks of the lakes there grew in an early stage of the Miocene epoch vast numbers of forest-trees and evergreen shrubs, of genera such as are now partly characteristic of tropical and sub-tropical countries; figs and vines, many species of Protoaceæ analogous to those that still grow in the Australian continent, together with cypress, sequoia, cinnamon, fan palms, and palmettos, ferns, hornbeams, and buckthorns, all of genera still familiar, but mostly if not altogether of extinct species. At a later date this vegetation partly died out, and was replaced by plane-trees, poplars, elms, willows, and maples; while cinnamons, figs, vines, laurels, and Protoaceæ still continued to flourish. In the woods, on the meadows, and in the waters respectively, the *Mastodon angustidens*, Rhinoceros, Chæropotamus, Dichobune, deer, Dinotherium, Hippopotamus, crocodiles, salamanders, fish, and numerous other creatures roamed at pleasure, while the air and the land were tenanted by dragon-flies, ants, beetles, and other insects, of which more than 800 species have been distinguished.

I now come to the chief part of this discourse, which is to account for the origin of the Rhine: for at that earlier time the Rhine had no existence in this valley; and indeed there is proof, that instead of the main drainage of the area flowing in a great river from south to north as it does now through this valley, the waters partly drained from north to south; and the pebbles of the Schwartzwald, instead of being carried north by the Rhine as they are now, were carried southwards by minor rivers, and found their way into Switzerland, thus helping to form some of the conglomerate rocks of which the Miocene strata of Switzerland to a great extent consist.

Not only had the Rhine no existence then, but the romantic gorge of the river, with which so many are familiar, had no existence either. It has been customary sometimes to attribute the formation of that gorge to violent disturbance and fracture of the strata, by which the waters were allowed to escape from south to north. I have no belief in such violent disturbances having any place in the modern economy of the world, nor yet in such cataclysmal action having ever affected the ancient world, as far as it is in the power of geologists to trace back events from the present day to the oldest known geological periods.

After the Miocene epoch had lasted for a long time, there occurred another disturbance of the European region, and of much of the rest of the world besides, though it is only the Alpine district and the countries north of the Alps that we have now to deal with. This second disturbance of the Alps produced a great upheaval of the Miocene strata. All the Miocene lakes that occupied the old lowlands of Switzerland, and extended far east into what are now the Austrian dominions,—all that area, as far as the Alps are concerned, was gradually heaved high above the level of the sea, and those beds of conglomerates, sandstones, and marls that form the lowlands of Switzerland, and all across what is now the Jura were disturbed to such an extent that the strata now forming the Righi and Rosberg and other sub-Alpine hills were partly raised to a height of at least 5800 feet above the level of the sea, and probably much more. The lower parts of Central Switzerland, about the Lake of Geneva, the Lake of Constance, and Neufchatel, still stand at heights of from 1200 to 1300 feet above that level. Then the range of the Jura first rose up to form a mountain-chain, and this is the proof of these disturbances. First we know that the Miocene rocks originally lay all the way from the Alps to the Taunus in flat-lying strata. During that period a vast quantity of Miocene pebbles were carried into the lakes, which were by and by consolidated into an exceedingly coarse conglomerate. Anyone who has ascended the Righi will remember that nearly the whole of it is formed of this coarse conglomerate, proving the prodigious amount of waste that the Alps underwent during the Miocene period. When we consider the amount of this waste, even though the waters of the Miocene period lay but little above the level of the sea, still in my opinion it is probable that the Alps themselves were then quite as high, if not higher, than they are now. For the amount of degradation proved by the conglomerate indicates the removal of an enormous amount of material from the pre-Miocene Alps.

After the disturbance which raised the Jura and the Miocene strata of the lowlands of Switzerland, this is what took place. Along with the contorted Secondary strata of the Jura, the Miocene beds that previously covered them were thrown into a number of anticlinal and synclinal curves, and the greater part of the Miocene material over that area having since been removed by denudation, only a few outlying fragments of these strata remain left in those wonderful upland basin-shaped hollows of the Jura, which still attest the original continuity of the Middle Tertiary deposits all the way from the base of the pre-Miocene Alps to the southern base of the Taunus.

When the post-Miocene disturbance of the whole of this area took place the general effect was, that while much of the Swiss Miocene area was contorted and raised high into the air, between the Jura and the Taunus the equivalent strata were simply heaved up and tilted, so as to form a long inclined plain sloping northerly between the Vosges and the Schwartzwald, and the surface of which may have been about 1200 or 1300 feet above the present level of the sea where Basel now stands, and about 1000 to 1100 feet high where the opening of the gorge now begins at Bingen.

Before this wide-spreading disturbance took place, the Rhine had no existence, for up to that time such small rivers as occasionally ran in the more ancient Miocene valley flowed partly south. But when the inclined plain was fairly completed, the result in the long run was that for the first time the great general drainage of the area began to run from south to north, and the Rhine was established, flowing at a height which we may roughly speak of as having been 500 feet higher than now, because at that time all the great valley between Basel and Bingen was filled to that height with Miocene strata. We know this to be a fact by an examination of the valley on the right hand and the left, from Bingen towards Basel; for every here and there we find table-shaped hills formed of flat-lying Miocene strata, which border the present alluvial plain of the Rhine, and abut upon the more ancient mountains on either side. The history revealed by this fact is plain to any one accustomed to reason on geological phenomena. The strata forming scarped slopes on opposite sides of the valley were once united, and their early continuity has been destroyed, simply by long-continued watery waste and denudation. They are indeed only the relics of an older phase of the physical geography of the district, when the surface of the plain stood about 500 feet higher than it does at present.

Now when the Rhine first began to flow, the river passed through a high upland valley with gently sloping sides that lay between the Taunus and the Hundsruck, and which in no manner resembled the precipitous cliffs that now bound the Rhine in the gorge below Bingen. The bottom of part of this old upland valley still forms a narrow terraced plain, immediately above and beyond the edge of the cliffy gorge of the Rhine. It is not always continuous on both sides of the gorge, but enough of it remains to attest its original continuity at heights of from 400 to 500 feet above the present level of the river. Now what I wish to persuade you of is this, that the Rhine flowing in this valley by degrees began to cut out its own gorge, and that it was not produced by fracture. Every running river is busy eroding its channel, especially where the ground is at all steep. That is one of the main functions of running waters. They are constantly deepening their channels, and carrying the sediments so formed from higher to lower levels, till in the course of time they find their way into lakes or the sea.

When we first enter the gorge of the Rhine, going southward, one feature that strikes the geological observer is the constant recurrence of this old terrace backed by the hilly country beyond. On the left bank, overlooking Bingen, the flat-topped spur of the Rochus-berg, about the same height as the tops of the neighbouring Miocene tabular hills, first strikes the eye. When fairly within the gorge below Niederheimbach, beyond its upper edge the old river-plain is seen gently sloping to the north, while the sides of the gorge itself are seamed by numerous gullies worn by occasional torrents since the great ravine—a kind of cañon—has been cut down to its present level.

At Welmich, below Niederheimbach, looking down the river, the edge of the terraced plain is seen receding northward in long perspective, and at Salzig, still further down, the features so well shown near Niederheimbach are again reproduced. The same outline occurs again and again all down the river between Bingen and Coblenz, and equally below Andernach, as for instance at Rheineck. Finally, above the Siebengebirge, just about the mouth of the gorge, looking up the river, the long eastern hills sloping to the river end in a terrace corresponding in general height and outline to those already mentioned. The general conclusion to be drawn from these observations is that at heights of from 450 to 500 feet above the present river this ancient river terrace has a persistent gentle slope from south to north, which approximately corresponds to that of the existing river.

The inference is plain: that formerly throughout the length of what is now the gorge the river flowed over that high-terraced level, at a time when the plain above the gorge was so deeply filled with Miocene strata that the level of the river, where Mainz and Bingen now stand, was as high as the upland terrace that crowns the gorge between Bingen and Rolandseck. By degrees the river began to excavate the gorge, slowly cutting deeper and deeper, and at the same time winding and ever changing its channel through the great plain between the Jura and the Taunus, by slow gradation it wasted away the surface of that plain more and more, and the matter won from that surface was carried down through the gorge to be added to the old delta of the river. At last the major part of the Miocene rocks that partly occupied the plain were worn away, and the plain has been thus reduced to its present temporary level; while the terraced hills on either bank still remain to attest the amount of watery degradation that the area has undergone.

So much for the scooping out of the valley. But there is another point which I would like to impress upon you. On each side of the Rhine there are important tributary rivers. Thus, for example, above the gorge we have the Maine, the Neckar, the Kinzig, the Elz, and other streams, flowing through deep steep-sided valleys; and these rivers have from a very early period been tributaries of the Rhine. It follows, then, that when the level of the Rhine was 400 or 500 feet higher than at present, the levels of the bottoms of these rivers must also have been 400 or 500 feet higher than at present; and therefore, just in proportion as the great valley of the plain of the Rhine was being cut down and lowered, so in proportion must the valleys in which these rivers run have gradually been deepened. When we come to the gorge, the same kind of argument applies to the Moselle and other tributaries of the Rhine.

I have elsewhere attempted to show that at one time the Moselle ran as high as the top of the table-land that now bounds it on each side. Everyone who knows that river is aware that, though it looks so hilly when we go up the stream in a steam-boat, as soon as we reach the edges of the slopes on either side, we are on the top of a great table-land intersected by numerous valleys, so that before the gorge of the Rhine was formed the Moselle ran at as high a level as the ancient Rhine; and just as the gorge of the Rhine was being deepened, so the Moselle was by degrees also enabled to deepen its channel. The same was the case with other rivers, right and left of the Rhine; and by applying this principle to the other great rivers of Europe we may hope in the long run to explain the physical history of the systems of drainage of all parts of the continent.

One other point remains to be stated with regard to the physical history of the Rhine. Geologists well know that in older times the glaciers of Switzerland were on an immensely larger scale than at present. Large as they appear to us at the present day, they are of pigmy size when compared with their magnitude at a comparatively late period of the world's history. The Rhone glacier then spread across all the area now occupied by the Lake of Geneva, till it abutted on the Jura; and the old Rhine glacier extended all over the Lake of Constance, and reached at least half-way from Schaffhaussen to Basel. The body of water which flowed directly from such glaciers must have been very great, and enormous must the moraines have been that were shed from the ice-sheets. From an examination of the pebbles that form the superficial gravel on the present plain of the Rhine below Basel, it is certain that a large portion of them have come from the Alpine regions. Such a great moraine as was shed from the western edge of the old glacier of the Rhine was constantly being attacked by the waters that flowed from its end, and thus by degrees pebbles were carried onward into the plain. The result is that a large part of the gravel of the Rhine is simply the waste of old moraines shed from the glaciers of Switzerland, added to by material carried down by the streams of the Vosges and the Schwartzwald, also partly derived from the moraines of ancient glaciers on a smaller scale.

Last year it was my lot to deliver here a discourse on the history of old continents, and I attempted to show that one old continent in particular retained its identity through a very long period of geological time; that from the close of the Upper Silurian period, all through the Old Red Sandstone and Carboniferous periods, through the Permian and New Red Sandstone epochs, over great part of what is now Europe, that continent, with many physical changes, still retained its identity. Such a vast continent remaining through all those geological periods implies a succession of epochs of time, which, as far as years and cycles of years are concerned, the mind has as yet only hints of data which some day may help us to grapple with such a problem, and not till astronomy comes more boldly to the help of geology may we begin to hope for the solving of the problem of the actual value of geological time. However that may turn out, it is certain that during the long continental epoch alluded to there were, over and over again, several changes in physical geography far greater than that petty change which I have been endeavouring to sketch out to-night. The floras and faunas of the world in that old time changed, not in the minor degree I have been speaking of to-night, but were more completely remodelled again and again in great part, even generically. Mountain ranges rose, glacial periods intervened and passed away. Great lakes, sometimes fresh, sometimes salt, appeared and were obliterated by successive terrestrial changes. At one time vast lakes, like those of the heart of Africa and North America, covered prodigious areas of land; at another, equal or larger areas were covered by salt lakes as large as the Caspian and the Sea of Aral. And when you think of the continental episode in the modern geological history of Europe to which I have drawn attention, you will see how small it really is, though it may look large to our minds, compared to the old continental epoch of which I spoke last year. This you may depend upon, that though to the superficial eye it may seem as if the world had always been going on just as it is doing now, and that through all time to come it will go on just the same, with its mountains, valleys, rivers, lakes, and seas, yet it is none the less certain that changes, such as I have described to-night, are but the forerunners of other mutations as great, aye, and far greater, that will take place in the future. Just as there is as yet no certainly *measured* limit to the geological time of the past, so also we know of no measurable limit to geological time to come. But why should I keep you with words such as these, when I may convey a whole chapter in physical geology, condensed into eight lines, by the greatest of our living poets :—

There rolls the deep where grew the tree:
  O Earth, what changes hast thou seen!
  There, where the long street roars, hath been
The stillness of the central sea.

The hills are shadows, and they flow
  From form to form, and nothing stands:
  They melt like mist, the solid lands,
Like clouds they shape themselves and go.

[A. R.]

## GENERAL MONTHLY MEETING,

Monday, April 6, 1874.

WARREN DE LA RUE, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

Frederick Albert Bosanquet, Esq.
Miss Brandreth,
Edward Brandreth, Esq.
Robert Bartholomew Lawes, Esq.
Robert Nicol, Esq.
William Wyndham Portal, Esq.
Edward Lake Walker, Esq.
John Witts Allen Woodroffe, Esq.

were *elected* Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

*Antiquaries, Society of*—Archæologia, Vol. XLIII. Part 2; Vol. XLVI. Part 1. 4to. 1873.

*Asiatic Society of Bengal*—Journal, 1873, Part 1, No. 3; Part 2, No. 3. 8vo.
Proceedings, 1873, Nos. 9, 10. 8vo.

*Astronomical Society, Royal*—Monthly Notices, Vol. XXXIV. No. 4. 8vo. 1874.

*Bavarian Academy of Sciences, Royal*—Abhandlungen, Band XI. Abth. 2. 4to. 1873.
Annalen der Kön. Sternwarte bei München. Band XIX. 8vo. 1873.

*British Architects, Royal Institute of*—Sessional Papers, 1873–4, No. 7. 4to.

*British Museum Trustees*—Catalogue of Hemiptera Heteroptera, Part 8. 8vo. 1873.

*Chambers, George F. Esq. M.R.I. (the Author)*—Record of Parliamentary Elections, 1868–74. 8vo. 1874.
Digest of the Law relating to Public Health and Local Government. Sixth Edition. 8vo. 1874.

*Chisholm, H. W. Esq. Warden of the Standards*—Reports of the Standard Commission. fol. 1868–73.
Annual Reports of the Warden of the Standards, 1–7. 8vo. 1867–73.
Report on the International Metric Commission at Paris. 1869–72. 8vo. 1873.

*Cox, E. W. Esq. Serjeant-at-Law, M.R.I. (the Author)*—What am I? Vol. II. 12mo. 1874.

*Croll, James, Esq. (the Author)*—On Ocean Currents, Part III. (Phil. Mag. Feb. 1873.)

*Editors*—American Journal of Science for March, 1874. 8vo.
Astronomical Register for March, 1874. 8vo.
Athenæum for March, 1874. 4to.
Chemical News for March, 1874. 4to.
Engineer for March, 1874. fol.
Food Journal for March, 1874. 4to.
Journal of Applied Science for March, 1874. fol.
Journal of Gas-Lighting for March, 1874. 4to.
Nature for March, 1874. 4to.
Nautical Magazine for March, 1874. 8vo.
Once a Week for March, 1874. 8vo.
Pharmaceutical Journal for March, 1874. 8vo.
Telegraph Journal for March, 1874. 8vo.

*Franklin Institute*—Journal, No. 577. 8vo. 1874.

*Geological Institute, Imperial, Vienna*—Jahrbuch, 1873. No. 4. 8vo.
Verhandlungen, 1873. No. 14.
Abhandlungen, Band VI. Theil 6. fol 1873.

*Incorporated Law Society of the United Kingdom*—Supplement to the Catalogue of the Mendham Collection. 8vo. 1874.

*Linnean Society*—Proceedings, Feb. 1874. 8vo.

*Manning, Frederick, Esq. M.R.I.*—Boscobel; or, the Royal Oak: a Tale of the year 1651. By W. H. Ainsworth. New Edition. 8vo. 1874.

*Mechanical Engineers' Institution, Birmingham*—Proceedings, July, 1873. Part 2. 8vo.

*Meteorological Office*—Proceedings of the Meteorological Congress at Vienna. 8vo. 1874.

*Photographic Society*—Journal, No. 248. 8vo. 1874.

*Praia da Victoria, Comte da, Gouverneur Civil de Ponta Delgada, Açores*—Les Eaux Thermales de l'Ile de San Miguel Açores. (K 99) 8vo. Lisbonne, 1873.

*Preussische Akademie der Wissenschaften*—Monatsberichte, Jan. 1874. 8vo.

*Rossetti, Professor F. Venice*—Sul Potere Specifico Induttivo dei Coibenti: ed Uso dello Macchino di Holtz in alcune Richerche Elettrometriche. (K 99) 8vo. 1873.
Discorso in Morte del Car F. Zantedeschi. (K 92) 8vo. 1873.

*Royal Society of London*—Philosophical Transactions for 1873, Part 2. 4to. 1874.
Proceedings, No. 150. 8vo. 1873.

*Tyndall, Professor, D.C.L. F.R.S. M.R.I. (the Author)*—La Chaleur, Mode de Mouvement; traduite par M. l'Abbé Moigno. 16to. Paris, 1874.

*Victoria Institute*—Wm. Forsyth on the Rules of Evidence as applicable to the Credibility of History. 8vo. 1874.

## WEEKLY EVENING MEETING,

Friday, April 17, 1874.

The Duke of Northumberland, D.C.L. President, in the Chair.

William Spottiswoode, Esq. LL.D. Treas.R.S. Sec.R.I.

### *On Combinations of Colour by means of Polarized Light.*

It is well known that if a beam of polarized light, which has passed through a plate of crystal, be analyzed with a double-image prism, two images will be seen tinted with complementary colours. The fact that the colours are complementary may be established by causing the images to overlap, when the part of the field of view common to both will be found to be white. And further, if the plate be of quartz cut perpendicularly to the axis, the images will change colour by a continuous sequence of tints when either polarizer or analyzer is turned continuously round. The colours so seen are, however, not simple colours of the spectrum, but mixed or residual tints, *viz.* the mixture of all those which remain when one or more have been extinguished. This is a direct deduction from the principles of the wave theory of light; but it was also practically demonstrated in some experiments which I had the honour of laying before the Institution last year. And, in fact, if we use a quartz plate of suitable thickness, and examine the spectrum of either of the images formed by the analyzer, we shall find it crossed with a single dark band which moves along the spectrum in one direction or the other, according to the sense in which either polarizer or analyzer is turned.

This principle enables us to make use of polarized light for investigating the subject of complementary colours. For if we form the spectra of the two complementary images, we shall find in general that the dark band occupies one position in the ordinary, and another in the extraordinary image. But since the two images are complementary to one another, and the part extinguished in each is complementary to the part which remains, it follows that the part extinguished in one is the complementary of the part extinguished in the other. That is to say, the bands in the two spectra will always mark out two complementary portions of the spectrum. It must, however, be borne in mind that the bands are not sharply defined like the Fraunhofer lines in the solar spectrum, but they are bands with a core of minimum or zero illumination shading off gradually on either side; so rapidly, however, that at a short distance from the core the adjoining colours appear in their full natural intensity. This being so, the tint suppressed in any band will be a compound of portions of all the colours comprised within the visible limits of the band. Of the colour corresponding to the core the whole will be suppressed, but of those on each side only portions, smaller and smaller as we recede from the core. And if we use the term "colour suppressed" in this sense, the pairs of colours determined by this method will be found to agree in the main with those given by Helmholtz as the result of the superposition of two spectra at right angles to one another, *viz.*:—

| | |
|---|---|
| Yellow-green, | Violet; |
| Yellow, | Indigo-blue; |
| Orange, | Cyanic-blue; |
| Red, | Green-blue. |

It will be noticed that, when one band arrives at the green moving in the direction of the red, the other or complementary band is just disappearing in the red; but at the same time another band is beginning to appear in the violet. We cannot therefore say that the green has for its complementary the red rather than the violet; the complementary in question is in fact a mixture of the two, a mixture which, when taken alone, is a reddish purple. This result agrees with the conclusion derived by Helmholtz from his own experiments.

Starting from the principle of these interference bands of polarized light, it occurred to me that if two or more such bands could be formed, and could be made to move independently and at pleasure in either direction along the spectrum, we should have the means of at once determining, and exhibiting, at all events the tints complementary to a combination of two or more bands chosen at will from the spectrum. And pursuing this subject further, I found not only that this is possible, but succeeded, by a combination of double-image prisms, actually to form in the field of view the tints complementary to the abovementioned complementaries; in other words, tints which represent the mixture of the two portions of the spectrum suppressed in the bands. For this purpose I have used, instead of the ordinary polarizing apparatus, the following arrangement, reckoned from the source of light towards the eye or screen. First, a Nicol's prism N as polarizer, then a quartz plate Q, next a double-image prism P; after that a second quartz plate $Q_1$, and finally a second double-image prism $P_1$. By this means four images are formed in the field of view—say, O O, O E, E O, E E, whereof O O, O E, occupy the positions of the two images formed by P alone; and O O, E O, the positions of those formed by $P_1$ alone. If for a moment we remove the second quartz plate, the arrangement above described will furnish some interesting experimental illustrations of the conclusions stated by Helmholtz—*viz.* that the low tint colours (couleurs degradées) such as russet, brown, olive-green, peacock-blue, are the result of low illumination. He mentions that he obtained these effects by diminishing the intensity of the light in the colours to be examined, and by, at the same time, maintaining a brilliantly illuminated patch in an adjoining part of the field of view. In accordance with what was said before, in the case where no quartz plate was used, it appears that by turning the prism $P_1$ round, we have the power of diminishing to any extent we please the intensity of the light in one pair of the complementary images, and at the same time increasing that in the other pair. We therefore have an equivalent of the conditions of Helmholtz's experiments. The conclusions were verified by throwing on the screen the spectra of two similarly tinted images, one highly, the other feebly illuminated, when it was found that the band occupied the same position in both spectra.

If a second quartz plate $Q_1$ be re-inserted between the prisms $P, P_1$, there will be 15 combinations of colour to consider, *viz.* 4 single images, 6 overlaps of two, 4 overlaps of three, and 1 overlap of all four images.

The tints of the four single images will now be those due to the formation of two bands in spectrum of each, *viz.* one due to the combination $Q\,P$, the other to the combination $Q_1\,P_1$. It will therefore be convenient in this case to designate their tints by the lines of letters representing the bands; thus, $B + A$, $B + A'$, $B' + A$, $B' + A'$; in which it is to be understood that $A, A'$ represent the pair of complementary bands formed by $Q\,P$, and $B, B'$ the pair extinguished by $Q_1\,P_1$.

The following is an arrangement of the images formed by P and $P_1$, together with the tints corresponding to the bands in each:—

$$\begin{array}{ccc} & OO & \\ & B + A & \\ EO & & OE \\ B' + A & & B' + A' \\ & EE & \\ & B + A' & \end{array}$$

OO represents the ordinary image of P and $P_1$; OE the ordinary image of $P_1$ and the extraordinary of P; &c. It is to be observed that $B + A$, $B' + A'$, occupy the positions of the two images formed by P alone; and $B + A$, $B' + A$, those of the images formed by $P_1$ alone. And it would at first sight appear that the colours suppressed in the first-mentioned images would be $B + A$, $B + A'$; but it must be remembered that the vibrations in the two images O, E, formed by P are at right angles to one another, and consequently the instrumental combination $Q_1\,P_1$ will in forming its ordinary image of A suppress the colour B, but in the ordinary image of A′ it will suppress its complementary B′. A similar remark applies to the other pair of images.

The effect of the combinations $Q_1\,P_1$ is to divide the images O O, O E, respectively into two parts, in one of which the colour B, in the other B′, is suppressed. Hence the overlap of $B + A$ and $B' + A$, will reproduce the original colour minus only the complementaries B, B′. This overlap may consequently be formularized thus,

$$B + A + B' + A = B + B' + 2\,A.$$

Similarly we may write

$$B + A' + B' + A' = B + B' + 2A'.$$

In other words, these two overlaps will be tinted with the colours A, A′. This may be verified by taking away the combination $Q_1 P_1$. And further, since the vibrations in the images $B + A$, $B' + A$, are at right angles to one another (as likewise are those in $B + A'$, $B' + A'$), it follows that when the polarizer N is turned round and the images change colour, the complementary character of the overlaps in question will always be maintained.

The same will be the case with the overlaps $B + A$, $B + A'$; and $B' + A$, $B' + A'$. But in order to verify this in the same way as in the former case, *viz.* by taking away the combination Q P, it must be borne in mind that the original plane of polarization must coincide with that of one of the images produced by $Q_1 P_1$. And it may be further noticed that if the original plane of polarization be altered by turning the Nicol N alone, the angle between the principal planes of P and $P_1$ will remain the same; and consequently the overlap in question will continue of the same colour whatever be the position of N.

Lastly, as regards the overlaps of $B + A$ and $B' + A'$, and of $B' + A$ and $B + A'$. It is clear that in both cases the images will have been deprived of the two pairs of complementary colours A, A′, B, B′; and therefore if the suppressed colours were all of the same intensity the overlaps would be uniformly white or grey. But as this is not the case, the parts in question will in general be tinted with the colours complementary to the feeblest of those suppressed. In fact, in the images

$$B + A \text{ and } B' + A',$$

if A be a feeble colour, A′ will in general be strong, and excepting when the reverse is the case with B and B′, the tint of $B + A$ will depend mainly on that produced by Q P, while that of $B' + A'$ will depend mainly on that produced by $Q_1 P_1$. Again, if in the same images, B approaches to A′, then will B′ approach A, and the two images will approach the same tint; while on the other hand, under the same circumstances the images

$$B + A' \text{ and } B' + A$$

will approach to complementary tints. In proportion as the common colour of the two first-mentioned images approaches to a grey, so will the overlap of the two last mentioned approach to white. In other cases the two overlaps in question will be of complementary tints.

We might apply the same train of reasoning to the triple overlaps, but the main interest of these parts of the figure consists in this, that each of the triple overlaps is complementary to the fourth single image not comprised in it; *e.g.* $B + A$ is complementary to $B' + A + B + A' + B' + A'$; and similarly for the others. This follows from the obvious fact that the quadruple overlap of all four single images is necessarily white. But the colours suppressed in each single image are together complementary to the colour of the image. Hence the colour of any triple overlap is the same to the eye as the mixture of the two colours suppressed in the outstanding single image. But since by turning the Nicol N we can displace the two bands in any way we please, and also by turning $P_1$ we can displace one of them in any way, we have the power of suppressing any two portions of the spectrum at will, and consequently of exhibiting to the eye the result of the combination of any two such portions.

A further step in the combination of colour may be made by using a third quartz $Q_2$, and a third double-image prism $P_2$, which will give rise to eight images; and using symbols similar to those used before, we may represent the tints suppressed by $Q_2 P_2$ by the letters C C′. The number of tints suppressed in each image is consequently three; and by seeking out the compartments which give the complementaries of each of the eight images, we shall have the representations of the three tints suppressed. It is hardly necessary to point out that by causing different parts Q P, $Q_1 P_1$, $Q_2 P_2$, of the instrument to rotate, we can suppress any three tints at will. The formulæ for the eight images will be

$C + B + A$ or O O O<br>
$C + B' + A'$ „ O O E<br>
$C' + B' + A$ „ O E O<br>
$C' + B + A'$ „ O E E<br>
$C' + B + A$ „ E O O<br>
$C' + B' + A'$ „ E O E<br>
$C + B' + A$ „ E E O<br>
$C + B + A'$ „ E E E

The number of combinations of tint as given by the compartments of the complete figure is as follows:—

$\frac{8}{1}$ = 8 single<br>
$\frac{8.7}{1.2}$ = 28 double<br>
$\frac{8.7.6}{1.2.3}$ = 56 triple<br>
$\frac{8.7.6.5}{1.2.3.4}$ = 70 quadruple<br>
$\frac{8.7.6}{1.2.3}$ = 56 quintuple<br>
$\frac{8.7}{1.2}$ = 28 sextuple<br>
$\frac{8}{1}$ = 8 septuple<br>
1 = 1 octuple

or 255 in all. It is perhaps unnecessary to follow these combinations out in detail, especially as the most interesting feature consists in the fact that the subjoined pairs are complementary to one another, viz.:—

| | |
|---|---|
| OOO<br>$C+B+A$ | EOE<br>$C'+B'+A'$ |
| EOO<br>$C'+B+A$ | OOE<br>$C+B'+A'$ |
| EEO<br>$C+B'+A$ | OEE<br>$C'+B+A'$ |
| EEE<br>$C+B+A'$ | OEO<br>$C'+B'+A$ |

Having this table in view, the application of the figure to the visible representation of the mixture of colours is simpler in this case than in that of four images; for to each single image corresponds as complementary another single image, and we can at once read off the results required without seeking any overlaps.

When the double-image prisms are so arranged that the separations due to P, $P_1$, $P_2$, are directed parallel to the sides of an equilateral triangle respectively, it is not difficult to read off the images in pairs. The scheme of images may then be disposed as follows, viz.:

| | | | |
|---|---|---|---|
| | OEO | OOO | |
| EEO | EOO | OEE | OOE |
| | EEE | EOE | |

And the pairs will be read off two horizontally and two vertically, by taking alternate images in each horizontal and in each vertical row, thus:—

OOO, EOE;
EOO, OOE;
EEO, OEE;
EEE, OEO.

[W. S.]

## WEEKLY EVENING MEETING,

Friday, April 24, 1874.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Charles Watkins Merrifield, Esq. F.R.S.

### *On Sea Waves.*

The speaker, in beginning his discourse, pointed out the special importance, in understanding the subject, of knowing that the advance of a wave is neither necessarily nor generally accompanied by any corresponding advance of the water itself. There are many other forms of motion in which this advance of form only occurs; and he exhibited large models of screws, pointing out that when the barrel on which the screw is cut is simply turned round without advancing, the screw-thread appears to advance like a series of waves; but that when the screw works in a nut, so that the screw advances, then the screw-thread remains apparently at rest. In the same way, in sea waves which appear to advance rapidly the water has no sensible motion of advance, while in the series of waves often seen below a bridge or shoal in a rapid river the water does advance, and the waves stand still.

What really constitutes a wave is that each particle of water shall go through the same little dance of its own, while the successive particles "take up the dance" in regular order. There will then be a wave or series of waves, whether there be any general movement or current in the water or not.

As the motion of a troubled sea is too complex to be understood without first considering the simpler forms of wave-motion, the speaker selected for illustration the simple succession of waves called "swell," or smooth ground-sea of deep water. This, as is well known, consists of a series of long unbroken ridges of water, apparently moving with considerable speed and force; yet, what the particles of water are actually doing is simply going round and round in circles in vertical planes; these planes being at right angles to the lines of the ridges, and the diameters of the circles being the height of the wave from valley to crest. There is no appreciable advance of the water; the advance of the wave being due to the particles taking up the dance in regular succession. If a disk or circle be rolled along the foot of a wall, a nail sticking out from it anywhere between its centre and its edge would trace on the wall the same curves (only upside down) as the profiles of the wave. A screw-thread drawn in suitable perspective also gives the same curves.

After giving this general notion of the sea wave, Mr. Merrifield pointed out its internal mechanical structure, showing that it consists of a series of undulating stratified layers (forming horizontal planes when the water is still), which undergo no mixing with one another. In the same way the water may be also ideally separated into vertical columns, which again do not mix, but only sway to and fro like stalks of barley when the wind sweeps over the field of corn. Only in the sea waves the columns lengthen as the wave crest approaches and shorten after it has passed away, always leaning towards the crest of the wave. The swell or ground-sea of deep water thus simply heaves the mass of water without stirring or mixing it, and without any general motion of advance, notwithstanding the apparent rapid motion of the waves,

In this absolute form the kind of wave described is perhaps rather the limiting condition which the waves tend to assume when left to the action of gravity alone than a state of things absolutely realized. Still, the departure from it is much smaller than might be supposed; and the apparent confusion of motion, when once the wind ceases, is rather due to several systems of swell, of different dimensions, existing at the same time and place, than to any essential difference in the character of the motion. After some details respecting the combination of waves and their height and length, the discourse was concluded with some remarks on waves of shallow water, and on the formation of waves in a rising storm. The illustrations consisted of diagrams and models.

## ANNUAL MEETING,

### Friday, May 1, 1874.

**Warren De la Rue**, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1873 was read and adopted. This Report testifies to the increasing prosperity and efficiency of the Institution, and congratulates the Members on the fact that the New Laboratories have been constructed and fitted up by means of funds contributed by themselves; and that this heavy charge is not larger than the surplus income of a few years will probably be able to reimburse.

Fifty new Members were elected in 1873.

Sixty-two Lectures and Nineteen Evening Discourses were delivered in 1873.

The Books and Pamphlets presented in 1873 amounted to about 120 volumes, making, with those purchased by the Managers, a total of 259 volumes added to the Library in the year, exclusive of periodicals.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to the Professors, for their services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:-

**President**—The Duke of Northumberland, D.C.L.
**Treasurer**—George Busk, Esq. F.R.C.S. F.R.S.
**Secretary**—William Spottiswoode, Esq. M.A. LL.D. Treas.R.S.

**Managers.**

George Berkley, Esq. C.E.
Adm. Sir Henry John Codrington, K.C.B.
Warren De la Rue, Esq. D.C.L. F.R.S.
Sir T. Frederick Elliot, K.C.M.G.
Edward Frankland, Esq. D.C.L. F.R.S. Cor. M. Acad. Sc. Paris.
Francis Galton, Esq. F.R.S.
John Peter Gassiot, Esq. D.C.L. F R.S.
Cæsar H. Hawkins, Esq. F.R.S.
Alfred Latham, Esq.
John Carrick Moore, Esq. M.A. F.R.S. F.G.S.
Sir W. Frederick Pollock, Bart. M.A.
The Earl of Rosse, D.C.L. F.R.S.
Robert P. Roupell, Esq. M.A. Q.C.
C. William Siemens, Esq. D.C.L. F.R.S.
Colonel Philip James Yorke, F.R.S.

**Visitors.**

Joseph Brown, Esq. Q.C.
John Charles Burgoyne, Esq.
Alexander John Ellis, Esq. F.R.S.
Col. James Augustus Grant, C.B. F.R.S.
Lieut.-Gen Sir Frederick William Hamilton, K.C.B.
Thomas Hyde Hills, Esq.
Robert Pilkington Linton, Esq. F.R.C.S.
William Millar, Esq.
A. C. Brisbane Neill, M.D.
Wyndham Spencer Portal, Esq.
William Salmon, Esq.
John Bell Sedgwick, Esq.
James Spedding, Esq.
The Hon. John Gage Prendergast Vereker.
Hensleigh Wedgwood, Esq.

## WEEKLY EVENING MEETING,

Friday, May 1, 1874.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

PROFESSOR ROLLESTON, M.D. F.R.S.

### *The Early Inhabitants of the North of England.*

PROFESSOR ROLLESTON began his discourse by referring to various objects before him, such as urns and their contents, ornaments, and other relics, procured from the graves of the various races who successively inhabited the North of England. He then described the discovery of a very large number of Anglo-Saxon cremation urns in a part of the East Riding of Yorkshire which is connected with the names of Edwin, Paullinus, and Coifi; a fact affording strong confirmation of his opinion, propounded to the members March 25, 1870, that the Anglo-Saxons were exceedingly numerous in Britain. He also exhibited and described some sets of ornaments obtained by him in the same district from Anglo-Saxon burials, which afford evidence of a stage between Heathen cremation and Christian burial, as described by him in the 'Archæologia,' vol. xlii. 1870. With these relics some much older implements as well as others quite modern have been accidentally or purposely mixed up in the graves themselves, and have thus led, or may lead, to various fallacies.

A brief summary was then given of the way in which cremation sculpture in England during the period of Saxon heathendom, from A.D. 450 to 630, gave way in Roman Britain to inhumation, from the date of the generation subsequent to Septimus Severus down to A.D. 450.

Before passing to the description of the pre-Roman and prehistoric modes of interment practised in the North of England, the Professor made some remark on the rationale, or, as it may be termed, the genesis of cremation, and expressed his mingled assent and dissent to the recently published views of Professor Unger; and the account of the "Funeral Ceremonies of the Ancient Hindus," given by Babu Rajendralala Mitra in the 'Journal of the Asiatic Society,' No. 4, 1870.

Affection for a deceased relative may have suggested two objects to the survivors of all ages; firstly, the preservation of the surroundings of the deceased in the very condition in which they were at the time of death; this object being felt to be a right one, as the mourner did not realize the fact of death in all its bearings at once. Secondly, it was felt to be right to have the remains of the departed preserved under the protection and in the proximity of kinsfolk. The chambered long barrow, with its inmates ranged in a crouching position along its wall, having food and other things placed side by side with them, in imitation of the home tenanted by them in life, answered both purposes.

Difficulties of transport, death in war, or by plague, or the natural consequences of a hot climate, as described by Tiresias in the 'Antigone' of Sophocles, often made cremation the only possible mode of securing at least the second of these objects.

The Professor concluded with a description, illustrated by diagrams, of the round and long barrows of the North of England, at the examination of which by the Rev. Canon Wm. Greenwell he had many times assisted.

The following passage from Wordsworth's 'Prelude' (Book xiii.) is beautifully illustrative of the subject of the discourse:—

"Time with his retinue of ages fled
Backwards, nor checked his flight until I saw
Our dim ancestral Past in vision clear;
Saw multitudes of men, and, here and there,
A single Briton clothed in wolfskin vest,
With shield and stone-axe stride across the wold;
The voice of spears was heard, the rattling spear,
Shaken by arms of mighty bone, in strength,
Long mouldered, of barbaric majesty."

## GENERAL MONTHLY MEETING,

### Monday, May 4, 1874.

**The Duke of Northumberland**, D.C.L. President, in the Chair.

The following Vice-Presidents for the ensuing year were nominated—

Warren De la Rue, Esq. D.C.L. F.R.S.
Sir Frederick Pollock, Bart. M.A.
The Earl of Rosse, D.C.L. F.R.S.
George Busk, Esq. F.R.S. the Treasurer.
William Spottiswoode, Esq. LL.D. F.R.S. the Secretary.

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Miss Constance Charlotte Astley,
G. L. Basset, Esq.
William Morgan Brown, Esq.
Josiah Owen Corrie, Esq.
Henry Deacon, Esq. J.P. F.C.S.
Gordon Kenmure Hardie, M.D.
George James Sheridan Lock, Esq.
Thomas Frederick Miller, Esq.
Samuel Sampson, Esq.
Thomas Taylor, Esq.

were *elected* Members of the Royal Institution.

**John Tyndall**, Esq. LL.D. F.R.S. was re-elected Professor of Natural Philosophy.

The Managers announced that they had appointed **John Hall Gladstone**, Esq. Ph.D. F.R.S. Fullerian Professor of Chemistry in the room of Dr. **Odling**, resigned.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

**From**

*The Lords of the Admiralty*—Capt. G. S. Nares: Reports of H.M.S. 'Challenger.' fol. 1873.
*Agricultural Society of England, Royal*—Journal, Second Series, Vol. X. Part 1. 8vo. 1874.
*Astronomical Society, Royal*—Monthly Notices, Vol. XXXIV. No. 5. 8vo. 1874.

*Basel Naturforschende Gesellschaft*—Verhandluugen, VIter Theil, 1stes Heft. 8vo. 1874.
*Bentley, Professor, F.L.S.*—Lecture on Eucalyptus Globulus. (K 99) 8vo. 1874.
*Bombay Branch of Royal Asiatic Society*—Journal, Vol. X. No. 28. 8vo. 1873.
*British Architects, Royal Institute of*—Sessional Papers, 1873–4, Nos. 8, 9. 4to.
*British Museum Trustees*—Guide to Græco-Roman Sculptures. 16to. 1874.
*Chemical Society*—Journal for March, April, 1874. 8vo.
*Comitato Geologico d'Italia, R.*—Bolletini, Vol. IV. 8vo. Firenze, 1873.
*Editors*—American Journal of Science for April, 1874. 8vo.
Astronomical Register for April, 1874. 8vo.
Athenæum for April, 1874. 4to.
Chemical News for April, 1874. 4to.
Engineer for April, 1874. fol.
Food Journal for April, 1874. 4to.
Journal for Applied Science for April, 1874. fol.
Journal of Gas-Lighting for April, 1874. 4to.
Nature for April, 1874. 4to.
Nautical Magazine for April, 1874. 8vo.
Pharmaceutical Journal for April, 1874. 8vo.
Quarterly Journal of Science for April, 1874. 8vo.
Telegraph Journal for April, 1874. 8vo.
*Franklin Institute*—Journal, Nos. 578, 579. 8vo. 1874.
*Geographical Society, Royal*—Proceedings, Vol. XVIII. No. 2. 8vo. 1874.
*Harlem, Société des Sciences*—Archives Néerlandaises, Tome VIII. Liv. 3, 4. 8vo. 1873.
D. Mulder Bosgoed, Bibliotheca Ichthyologica et Piscatoria. 8vo. Harlem, 1873.
*Liverpool Polytechnic Society*—Transactions, 1853-6. 8vo.
Journal, 1865–70. 3 vols. 8vo.
*Manchester Philosophical Society*—Memoirs, Third Series, Vol. IV. 8vo. 1871.
Proceedings, Vols. VIII. IX. X. XIII. Nos. 7-11. 8vo. 1869–73.
*Martin, James, Esq. F.M.S. (the Author)*—The Magic Lantern. 16to. 1870.
*Mechanical Engineers' Institution, Birmingham*—Proceedings, Oct. 1873. 8vo.
*Medical and Chirurgical Society, Royal*—Proceedings, Vol. VII. No. 5. 8vo. 1874.
*Musée Teyler*—Archives, Vol. III. Fasc. 3. 8vo. Harlem, 1873.
*Photographic Society*—Journal, No. 249. 8vo. 1873.
*Preussische Akademie der Wissenschaften*—Monatsberichte, Feb. 1874. 8vo.
*Rolleston, Professor, M.D. F.R.S.*—Photograph of Urns from Barrows in Yorkshire.
*Royal Society of Edinburgh*—Transactions, Vol. XXVII. Part 1. 4to. 1873.
Proceedings, No. 85. 8vo. 1873.
*Royal Society of London*—Proceedings, No. 151. 8vo. 1874.
*Royal Society of Tasmania*—Monthly Notices, 1872. 8vo. 1873.
*Schwarz, F. M. Esq.*—A. Raimondi: El Departmento de Ancachs y sus Riquesas Minerales. 4to. Lima. 1873.
*Statistical Society*—Journal, Vol. XXXVII. Part 1. 8vo. 1874.
*Victoria Institute*—Journal, No. 28. 8vo. 1873.

## WEEKLY EVENING MEETING,

Friday, May 8, 1874.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Sedley Taylor, Esq. M.A.

### *An Historical Enigma in the Trial of Galileo before the Inquisition.*

[Abstract.]

The best informed historians who have hitherto treated of the condemnation of Galileo by the Inquisition are unanimously of opinion that the Tribunal, whatever may be thought of the wisdom of its proceedings, had a clear *legal right* to punish the Italian philosopher for the course he pursued.

The object of the present Discourse is to show that fresh evidence, brought to light within the last eight years, renders it all but certain that the Court was technically in the *wrong*, and the accused entitled to an unconditional acquittal. A rapid survey of the evidence at the disposal of historians up to the year 1850 will serve as a convenient introduction to a description of the sources of information opened since that time. Galileo was born in 1564. In 1589 he was appointed to a Professorship at Pisa, which in 1592 he exchanged for a similar post at the University of Padua. It was probably about this time that he became a convert to the heliocentric system of astronomy, which had been published to the world by Copernicus in his great work, *De Revolutionibus Orbum Cœlestium*, in 1543. For some years Galileo seems to have observed a policy of reticence on the subject, and in his public lectures to have gone on teaching the old Ptolemaic system. By 1615, however, he must have allowed his genuine convictions to appear, for in that year the monks of the Monastery of St. Mark at Florence proceeded to the extreme measure of denouncing him to the Inquisition. The result of the step was that on March 5, 1616, the Congregation of the Index issued a decree pronouncing the heliocentric theory to be false and contrary to Holy Scripture, and ordering the withdrawal from circulation of the work of Copernicus, until such time as certain alterations in its text should have been decided upon by the Congregation, and effected under their orders. This Index decree, and the list of corrections which appeared four years after it, are of great historical importance as determining the attitude which astronomers who were members of the Roman Church were required henceforth to assume towards the new theory. A careful examination and comparison of these two documents leaves no manner of doubt as to the extent of the restrictions which the ecclesiastical authorities were resolved to impose. All theological argumentation on the side of the new theory was to be peremptorily silenced. On the other hand, writers who adopted a purely scientific mode of treatment were not debarred from producing philosophical arguments which made in favour of the revolutionary astronomical doctrine, but they were prohibited from expressing belief in its truth, and required to couch all their conclusions in hypothetical language.

In 1632 Galileo published his famous 'Dialogue on the Ptolemaic and Copernican Systems.' This work presented, in a popular and singularly attractive dress, the case of the new against the old astronomy. It caused a prodigious excitement among all classes of cultivated men, gave the most mortal offence to the school of Aristotle, and notably to Galileo's declared enemies, the Jesuits, and led directly to all the misfortunes and suffering which were destined to embitter the remainder of his life. The Dialogue was suddenly suspended by a peremptory order from Rome, and, two months later, Galileo was cited to appear personally before the Inquisition on a charge of heresy. Ill-health prevented his reaching the capital until the middle of February, 1633, and the Court did not pronounce sentence until the latter part of June in the same year. The prisoner was convicted on two main issues—

(1.) That the Dialogue transgressed the limits laid down by the decree of the Index.

(2.) That it was a violation of a special personal injunction, containing far more stringent restrictions than those of the public decree, which, as the Tribunal asserted, had been formally served on Galileo on the 26th of February, 1616, by the Commissary-General of the Holy Office.

The first allegation was so inherently feeble, that it could not even sustain a straightforward reference to the actual text of the Index decree; and the Court, in order to give it some apparent colour, were obliged to resort to the humiliating expedient of garbling the statute which they pretended to be enforcing. The answer to be made on behalf of the accused is complete and decisive. In the first place, Galileo, in writing his Dialogue, had most carefully adopted the attitude prescribed by the Index Congregation; and, secondly, even had he not done so, was completely shielded by the fact that he had submitted the work in manuscript to the Papal licensing authority, and obtained the regular *Imprimatur* before proceeding to publication.

The second allegation was a far more serious matter. The injunction (or precept, as it was technically described) which Galileo was asserted to have received from the Inquisition, contained a clause forbidding him to defend or teach the Copernican doctrine *in any manner whatever, either by word of mouth or in writing.* No one who is acquainted with the contents of the Dialogue can have any doubt that, though compatible with the terms of the Index decree, they are utterly inconsistent with the far stricter enactments of the private inhibition. Everything therefore turns on this question: *Was the precept actually delivered to the accused as stated by the Tribunal?*

It is a remarkable circumstance that subsequent historians have, one and all, accepted this unsupported assertion of the Court, although there lay on the surface of the final Sentence a glaring contradiction with respect to this crucial point of the whole trial. This contradiction is as follows. In the course of a summary of the events of the case with which the Sentence opens, it is stated that on February 25, 1616, Cardinal Bellarmine was commissioned by the Holy Office to send for Galileo and give him a private admonition to relinquish the Copernican tenet. In the event of his non-acquiescence, the Commissary of the Holy Office was to intervene with the formal precept already referred to. On the following day the precept was actually delivered by the Commissary, Cardinal Bellarmine being, it is explicitly stated, *present throughout the proceeding.* Later in the Sentence, when the defence set up by the accused has to be examined, an autograph certificate given to Galileo by Bellarmine is cited. The Cardinal certifies that *no injunction exceeding in stringency the decree of the Index* had, as far as he was aware, been laid on the accused. These statements are clearly inconsistent with each other, and we here, therefore, have the Inquisition in 1633 contradicting a former leading member of their own body on a matter of fact which they themselves assert to have occurred in his immediate presence. This conflict of evidence is the "Enigma" in Galileo's trial referred to in the title of this Discourse.

No fresh light was thrown on this subject till 1850, when *Marino Marini*, Prefect of Papal Archives, published a certain number of extracts from the official minutes of the trial preserved in the Records of the Inquisition. His work has since been superseded, and is now only interesting as showing that its author had recognized the Enigma and thought it desirable to submit the documents which revealed it to a series of manipulations designed to efface all traces of the unwelcome indications which, in their original form, they contained.

In 1867 *M. Henri de l'Epinois* was allowed by the Roman authorities to publish *in extenso* the greater part of the documents from which Marini had given only meagre, disjointed, and untrustworthy extracts. These embrace the text of the minute on the strength of which the Inquisition asserted the delivery of the Commissary's precept, a verbatim report of Galileo's examinations during the trial, and a copy of his defence handed in in writing to the Tribunal.

In 1870 *Professor Silvio Gherardi* published a number of fresh documents, of the existence of which nothing was previously known, and which he discovered during a search in the Archives of the Inquisition, instituted during the short suspension of the temporal power in 1848 and 1849. One of these, copied by Signor Gherardi with his own hands from the original, has a singularly cogent bearing on the special point under consideration, as will presently be seen.

*Herr Emil Wohlwill*, of Hamburg, in a pamphlet published in 1870, had the distinguished merit of for the first time clearly stating the nature of the Enigma of the trial, examining the inferences to be drawn from De l'Epinois' new documents with respect to it, and advancing a theory of his own for its solution. The conclusion he has arrived at is, that *the precept of* 1616 *was not delivered to Galileo at all*, and that the minute asserting its delivery is a *fraudulent fabrication*, perpetrated in order to secure a condemnation which could not be obtained by fair means. It is a satisfactory circumstance that the principal document unearthed by Gherardi, and published *after* the appearance of Wohlwill's pamphlet, confirms the above conclusion in the most striking manner, and indeed led Gherardi, in ignorance of Wohlwill's very existence, to a view substantially identical with his.

The trial minute, whose genuineness Wohlwill attacks, sets out the Commissary's professed execution of the instructions issued to him by his superiors, which are detailed in the preceding minute. The instructions provided that the Commissary was only to intervene in the case on the occurrence of a particular contingency, viz. Galileo's refusal to obey the Cardinal's admonition; whereas, according to the disputed minute, he seems to have delivered his message the moment Bellarmine had ceased speaking, and, as the language used seems to imply, without even pausing to ascertain whether Galileo intended to submit or not. This discrepancy can only be removed by assuming, without a tittle of direct evidence, that Galileo refused to acquiesce when admonished by the Cardinal, and so brought down upon himself the precept of the Commissary, an assumption of course quite inadmissible, especially on so vital an issue. On the other hand, if it can be shown either that Galileo submitted to the private admonition, or that the precept was not really delivered to him, the sole ground which could justify a conviction is destroyed, and the reasoning of the sentence falls hopelessly to pieces. Hence evidence in support of Wohlwill's theory will be equally to the point whether it goes to prove the first or the second of these propositions. The arguments now to be produced (which are nearly all due to the sagacity and research of Wohlwill) will be conveniently grouped under a corresponding arrangement, as showing

(1.) That Galileo acquiesced when admonished by Bellarmine.
(2.) That he did not receive the Commissary's precept.

Under the first head a certain weight is due to the fact that Galileo was a devout and warmly attached member of the Roman Church, and therefore very unlikely to have resisted an appeal from the most eminent Cardinal in Rome, who, besides being the leading member of the Holy Office at the time, had previously evinced a personally friendly interest in the philosopher. This presumption is strengthened by the fact that, in contemporary letters of Galileo, we find him congratulating himself on the docile and filial attitude towards the Church authorities which he had maintained throughout this whole affair. He repeats the same asseverations, almost in the same words, in a letter written two years after his condemnation, and dwells on them as constituting one of his main consolations under punishment which he was conscious of having done nothing to deserve. There was at this time no hope of obtaining any further mitigation of his sentence by professions of zeal for the Church, and no use in making them had they not been true and genuine.

Passing from merely presumptive arguments, we come to the document published by Gherardi to which reference has already been made. It relates how, on the 3rd of March, 1616, Cardinal Bellarmine reported in person that Galileo, being admonished to abandon the heliocentric position, *had acquiesced.* Unless this document can be shown to be spurious, its testimony in support of proposition (1) is conclusive.

In the second group of evidence a leading position is due to Bellarmine's certificate, written with his own hand and given to Galileo within three months of the interview. Its gist is that nothing more than the substance of the Index decree was communicated to Galileo on the occasion. The interview took place after the contents of the decree had been decided upon by the Congregation, but before its actual publication, so that the *rôle* of the Cardinal clearly was to give Galileo an early intimation of what was coming, and secure his promise of adhesion. The Court was evidently satisfied as to the genuineness of the certificate, and, this being so, there appears to be no way of avoiding the inference which follows from it negativing the delivery of the Commissary's precept.

A series of facts, derived principally from Galileo's correspondence, point in the same direction.

Within a fortnight of the interview with Bellarmine, Galileo had an audience of the Pope, who assured him how well he stood with himself and the whole Congregation of the Holy Office—an incident which could scarcely have occurred had he so recently resisted the leading Cardinal of their body, and, in consequence, been visited with a peremptory and threatening inhibition at the hands of their Commissary-General.

In all his writings on the Copernican doctrine subsequent to the decree of the Index, Galileo took especial care to conform to its requirements, and generally inserted an explicit statement of his adhesion. He must have been aware that this was a perfectly futile precaution if he was all the time violating more severe restrictions personally laid upon himself.

An extremely cogent argument against the reality of the Commissary's message is supplied by the history of the negotiations which preceded the publication of the great Dialogue. Galileo placed in the hands of the licensing authority at Rome a work the very title of which constituted a violation of the Commissary's precept. The manuscript, after being subjected to minute inspection, received the final approval of the licensers, whose *Imprimaturs* adorn the first page of the volume. Their conduct is perfectly intelligible on the assumption that *the Index decree alone guided their scrutiny.* On the opposite theory we can only account for it by the extremely improbable hypothesis that the Inquisition, after taking especial pains to bind Galileo over to absolute silence about the Copernican system, neglected to inform the licensers of the fact, and thus rendered futile the precautionary measures which they had themselves deemed it necessary to adopt.

The statements of Galileo during his trial are irreconcilable with the delivery of the Commissary's precept. In the interrogatory which took place on April 12, the examining official, who manifestly held in his hand the disputed minute, endeavoured, by reading aloud isolated portions of its text, to recall the incident it described to the memory of the accused, and elicit from him an admission of its truth. These efforts were fruitless. Galileo deposed that he had no recollection of a formal communication having been made to him on the subject by any other person than Cardinal Bellarmine. Further, in his written defence handed in to the Tribunal, he urged that the authentic attestation which he possessed in the Cardinal's certificate contained no vestige of an indication that any injunction had been laid upon him additional to the general decree of the Index, which concerned everyone alike.

These statements can only be set aside by hypotheses of defective memory, or of deliberate perjury. The former is out of the question, the cross-examination of the accused alone sufficing, had we no other evidence, to show that Galileo's memory at this period was singularly keen and retentive. To deny on oath facts as to which he knew from the lips of the presiding judge that the Court was in possession of a formally attested record, was a course so hopeless in itself, and so likely to lead to the immediate application of the torture, that it is impossible to attribute it to a man of ordinary circumspection, to say nothing of one possessing preeminent tact and sagacity.

This concludes the evidence to be now alleged in support of proposition (2).

The chain of facts and arguments above briefly set out constitutes the case by which Wohlwill claims to have proved that a supposititious document, fabricated for that very purpose, was the instrument which alone rendered possible the condemnation of Galileo. It will hardly be denied that the evidence he has collected renders it far more probable that the story of the Commissary and the precept of 1616 is a fiction than that it is a fact. The only chance of modifying this conclusion lies in the production of additional evidence, and it is the main object of this Discourse to appeal to those in whose custody such evidence unquestionably exists no longer to withhold it. In consequence of the disclosures spontaneously made by the Roman authorities through M. de l'Epinois, the Inquisition is brought under the gravest suspicion of having, in perhaps the most celebrated and momentous case ever brought before it, inflicted a cruel wrong on the ground of a gross fraud. Unless, therefore, they prefer the judgment of history to go by default, the custodians of the Vatican Library will do well to publish *in fac-simile* all the Galileo documents in their possession, so as, if possible, to bring this question to a decisive issue.

[S. T.]

## WEEKLY EVENING MEETING,

Friday, May 15, 1874.

The Earl of Rosse, D.C.L. F.R.S. Vice-President, in the Chair.

C. William Siemens, D.C.L. F.R.S. M.R.I.

### *The Steamship 'Faraday' and her Appliances for Cable-laying.*

The speaker in his introductory remarks observed that an electric telegraph consisted essentially of three parts, viz. the electro-motor or battery, the conductor, and the receiving instrument. He demonstrated experimentally that the conductor need not necessarily be metallic, but that water or rarefied air might be used as such within moderate limits; at the same time, for long submarine lines, insulated conductors strengthened by an outer sheathing were necessary to ensure perfect transmission and immunity from accident. The first attempts at insulation, which consisted in the use of pitch and resinous matters, failed completely, and in the years 1846 and 1847 the two gums india-rubber and gutta-percha were introduced, the former by Prof. Jacobi of St. Petersburg, and the latter by Dr. Werner Siemens of Berlin. This last gum soon became almost indispensable to the cable manufacturer on account of its remarkable plasticity at low temperatures and its insulating property.

The first outer sheathing used was a tube of lead drawn tightly over the insulated wire, and this again was covered with pieces of wrought-iron tubing connected by ball and socket joints; in this way the Elbe and other rivers were crossed successfully in 1848–50. This method was superseded later on by the spiral-wire sheathing, first proposed by Mr. Brett in 1851 for the Dover and Calais cable; since then, with few modifications and exceptions, this form has been universally adopted.

The speaker next enumerated the casualties to which submarine cables are liable, and the precautions employed to obviate them. He showed specimens destroyed by rust and the ravages of a species of teredo. On the Indo-European Cable line a curious case of fracture occurred; a whale, becoming entangled in a portion of cable overhanging a ledge of rock, broke it, and in striving to get free had so wound one end round its flukes that escape became hopeless, and so had fallen an easy prey to sharks, which had half devoured it when the grappling iron brought his remains to the surface. Other enemies to be dreaded are landslips, ships' anchors, and abrading currents.

The new Atlantic cable consists, for the deep-sea portion, of copper conductors, gutta-percha insulators, and a sheathing of steel wires covered with hemp; the shallow-water part consists of similar conductors and insulators sheathed with hemp, which in turn is covered with iron wire.

In paying out, no catenary is formed, as might be supposed, but the cable passes in a straight line from the ship to the sea-bottom—a proposition which the speaker demonstrated experimentally by means of a long trough with glass sides filled with water. The retaining force applied by the brake wheel should be equal to the weight of a piece of cable hanging vertically downwards to the bottom of the sea. In picking up, a catenary is formed, but a vertical position is the best, because it produces the least resistance.

From the peculiar nature of the service for which a telegraph-ship is required, it is evident that she must possess properties somewhat different from those of ordinary ocean-going steamers; thus speed is not so important as great manœuvring powers, which will enable her to turn easily in a small space, or by which she may be maintained in a given position for a considerable time. In the ship about to be described an attempt had been made to meet these requirements.

The 'Faraday,' of 5000 tons register, was built at Newcastle by the eminent firm of Messrs. Mitchell and Co. She is 360 feet long, 52 feet beam, and 36 feet depth of hold; there are three large water-tight cable tanks having a capacity of 110,000 cubic feet; these are each 27 feet deep; two are 45 feet in diameter, and one is 37 feet; they can take in 1700 miles of cable 1¼ inch in diameter. After the cable is coiled in, the tanks are filled up with water to keep it cool; for the speaker had found, when conducting experiments on the Malta and Alexandria cable with his electrical resistance thermometer, that heat was spontaneously generated in the cable itself, whereby its insulation was seriously endangered.

The 'Faraday' has stem and stern alike, and is fitted with a rudder at each end; both are worked by steam-steering apparatus placed amidships, and are capable of being rigidly fixed when required. She is propelled by a pair of cast-steel screw propellers 12 feet in diameter, driven by a pair of compound engines constructed with a view to great economy of fuel. The two screws converge somewhat, and the effect of this arrangement is to enable the vessel to turn in her own length when the engines are worked in opposite directions. On the voyage from Newcastle to London a cask was thrown overboard, and from this as a centre the ship turned in her own length in 8 minutes 20 seconds, touching the cask three times during the operation. This manœuvring power is of great importance in such a case as repairing a fault in the cable, as it enables the engineer to keep her head in position, and, in short, to place her just where necessary, in defiance of side winds or currents.

The testing-room of the electrician in charge is amidships, and so placed as to command the two larger tanks, while the ship's speed can be at all times noted on the index of a Berthon hydrostatic log.

The deck is fitted with machinery to be used in laying operations, which will be best described by tracing the path of the cable from the tanks to the sea. Let us begin with the bow compartment: the cable, which lies coiled round one of Newall's cones, begins to be unwound, passes up through an eye carried on a beam placed across the hatch, next over a large pulley fitted with guides, and by a second pulley is gently made to follow a straight wooden trough fitted with friction rollers, which carries it aft to near the funnels; here it passes through the "jockey," which is a device for regulating the strain, consisting of a wheel riding on the cable, which can be adjusted by a lever, and a drum fitted with a brake. Thence it passes on to a "compound paying-out and picking-up machine," which consists of a large drum provided with a friction brake, and round it the cable passes three times; it is also furnished with a steam-engine, which by means of a clutch can be geared on to the drum when required. Now, in paying out, the cable causes the drum to revolve as it runs over it, and the brakes regulate the speed as the vessel moves onward; but should a fault or other accident render it necessary to recover a portion, the drum is stopped and geared on to the engine, the ship's engines are reversed, the stern rudder fixed; and so what was formerly the bow is now the stern, while the little engine hauls in the cable over the same drum which before was used to pay it out; thus it is coiled back into the same tank whence it started. By this means the necessity of passing the cable astern before proceeding to haul it in is avoided. It was during this operation that an accident befell the Atlantic cable in 1865, causing its loss for the time.

The next apparatus is a dynamometer, consisting of three pulleys, one fixed, and the centre one, which rests on the cable, movable in a vertical plane; by this the strain is registered and adjusted. After passing this the cable runs into the sea over a pulley carried on girders and constructed so as to swing freely on an axis parallel to the length of the ship, so that, should the vessel make lee-way, the pulley will follow the direction of the cable, and thus friction and sharp bends are avoided. The bows are also fitted with a similar pulley, compound machine, and dynamometer. We see that by these devices the cable is kept perfectly under control, and should a fault be discovered a simple process of reversal of ship and machinery brings home the faulty portion.

Another great point is to keep the vessel trimmed and steady. For the former requirement nine separate water-tight compartments, including the cone in each tank, which also is hollow, are provided, so that water may be admitted as the tanks are emptied of cable, and thus the ship is kept trimmed. To ensure steadiness and avoid the rolling to which telegraph ships are subject, two bilge keels are set on at an angle of 45°; this was done at the suggestion of Mr. Wm. Froude, whom, said the speaker, "I have to thank for valuable advice and assistance on several new points connected with the 'Faraday.'"

A steam-launch is carried on deck, whence she can be lowered into the water with steam up, ready to land shore ends and perform other useful operations.

Another class of work for which the vessel is fitted is "grappling" for lost or faulty cable. In shallow seas this is a very simple operation, but in deep water it is rather a delicate matter, and requires the co-operation of two or even three vessels, so as to lift the cable without forming an acute angle, and thus to lessen the chance of fracture. A special rope, made of steel wire and hemp and of great strength, is provided for this work. Some specimens shown could bear strains up to 16 tons.

In conclusion, the speaker adverted to the late Professor Faraday, noticing the great services he had rendered to electrical science, his singleness of purpose, and the invariable kindness with which he had encouraged younger labourers in the same field. The friendly encouragement which he himself had experienced from him would ever remain a most pleasing remembrance. He had seized with delight on the present opportunity to pay a tribute to the honoured name of Faraday, and was happy to be able to do this with the full consent of the revered lady who had stood by the philosopher's side for forty years, while labouring under this very roof for the advancement of knowledge. The name of the vessel and her mission in the service of Science would combine, he thought, to create an interest in her favour in the minds of the members of the Royal Institution, and he hoped that on the morrow she would put to sea accompanied by the earnest wish, "God speed the 'Faraday.'"

[C. W. S.]

## WEEKLY EVENING MEETING,

### Friday, May 22, 1874.

George Busk, Esq. F.R.S. Treasurer and Vice-President,
in the Chair.

### Professor W. Kingdon Clifford, M.A. F.R.S.

### *On the Education of the People.*

The speaker quoted Mr. Herbert Spencer's classification of knowledge in the order of its worth, remarking that he was about to regard education not so much in the light of acquirement of knowledge as in that of learning to do something, and mainly of learning to use tools. Thus, language is the tool of thought whose use is taught in primary education; and the higher education is training in the use of those more difficult conceptions, familiarity with which as tools constitutes the culture of the highest minds. The question "What is the education of the people?" may thus be translated into "What are the people taught to do?"

In regard to the question, Who are the people? he adverted to the abstracts of the last Census of the population of England and Wales; and, taking 142 adult men, classified them into various kinds of workers:—On land, 27; on minerals, 22; in construction, 21; on materials and dress, 17; general labourers, 10; carriers, 10; indefinite, 5; mercantile, 4; servants, 3; grooms, 2; workers and dealers in food and drinks, 8; general dealer, 1; soldiers, 2; keepers of lodgings, 2; to which he added one magistrate, clergyman, doctor, lawyer, policeman, teacher, student, and sailor.

The class engaged in construction being the one most largely represented in London, the speaker proceeded to select as a typical example the makers of hand-rails to staircases, in order to show what is at present the education of the workshop. By means of models and diagrams, he showed that the construction of a hand-rail involves the solution of a difficult geometrical problem, which would puzzle the lower half of the Wranglers, besides considerable manual skill and artistic taste. The problem is to construct a curve of double curvature approximately by means of plane pieces. The problem was solved by Peter Nicholson, a workman, but a far simpler method has been introduced by another workman, John Weston. This trade is only exceptional, in the fact that its power of education has not been spoilt by division of labour, which in other trades has been carried to a very great extent by the introduction of machinery. It is of course impossible to resist this tendency; but its consequences may be avoided, if the unions will insist on the men being drafted at reasonable intervals from one part of the shop to another, so that they may become handy and manysided, instead of being fit only to mind a particular corner of a particular machine, as is too often the case now. Besides this, it is necessary that an organized training in the most important tools should be given to children. By this means they will be able on leaving school to get higher wages and more leisure to improve themselves. But the most important thing is, that our workmen should acquire the machinery of accurate thought, which is only to be got by training in geometrical and mechanical drawing. This ought to be regarded as one of the "R's," and should be taught to all children in all primary schools; being more important to workmen than learning to read and write.

## WEEKLY EVENING MEETING,

### Friday, May 29, 1874.

Sir Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

Arthur Penrhyn Stanley, D.D. Dean of Westminster,
CORRESPONDING MEMBER OF THE INSTITUTE OF FRANCE.

## *The Roman Catacombs as illustrating the Belief of the Early Christians.*

I propose this evening to ask what was the belief of the early Christians, that is, of the Christians from the close of the first century to the conversion of the Empire at the beginning of the fourth. This is a question which is at once more difficult and more easy to answer than we might have thought beforehand.

It is in one sense extremely difficult.

The popular, the actual belief of a generation or society of men cannot always be ascertained from the contemporary writers, who belong for the most part to another stratum. The belief of the people of England at this moment is something separate from the books, the newspapers, the watchwords of parties. It is in the air. It is in their intimate conversation; we must hear, especially in the case of the simple and unlearned, what they talk of to each other; we must sit by their bedsides; get at what gives them most consolation, what most occupies their last moments. This, whatever it be, is the belief of the people, right or wrong—this, and this only is their real religion. A celebrated Roman Catholic divine* of the present day has described, in a few short sentences, what he conceives to be the real belief of the people of England. He is probably right; but it is something quite apart from any formal creeds or confessions or watchwords which exist. Is it possible to ascertain this concerning the early Christians? Their books of that period are few and far between, and these books are, for the most part, the works of learned scholars rather than of popular writers. Can we apart from such books discover what was their most ready and constant representation of their dearest hopes here and hereafter? Strange to say, after all this lapse of time it is possible. The answer, at any rate for that large mass of Christians from all parts of the empire that was collected in the capital, is to be found in the Roman Catacombs.

It is not my intention to describe those well-known structures. To anyone who wishes to find a clear, brief, and impartial account, I recommend the essay "On Pagan and Christian Sepulture," in the 'Essays' of Dean Milman. For anyone who wishes to go at length into the details of the question, I refer to the great work of Commendatore De Rossi, or to his own personal exposition of the Catacombs, which (in my own case) has furnished the chief basis of these remarks. I take for granted that all here present know sufficiently what has been amply proved by the investigations of the last two hundred, and especially of the last thirty years, that there were in the neighbourhood of Rome from the first beginning of the settlement of the Christians in the city, large galleries dug in the rock, which they used for their places of burial. In these galleries they wrote on the graves of their friends the thoughts that were most consoling to themselves, or painted on the walls the figures which gave them most pleasure. By a singular chance these memorials have been preserved to us by the very causes which have destroyed so much beside. The Catacombs were deserted at the time of the invasion of the barbarians, and filled up with ruins and rubbish; and from the sixth to the seventeenth century no one thought it worth while to explore them. The burial of Christian antiquity was as complete as that of Pagan antiquity, and the resurrection of both took place nearly at the same time. The desertion, the overthrow of these ancient galleries has been to the Christian life of that time what the overthrow of Pompeii by the ashes of Vesuvius was to the Pagan life of the period immediately antecedent. The Catacombs are the Pompeii of early Christianity. It is much to the credit of the authorities of the Roman States that at the time when the excavations began they allowed these monuments to speak for themselves. Many questionable interpretations have been put upon them, but I am not aware that in any respect there has been substantiated any charge of wilful falsification.

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* Dr. Newman's 'Grammar of Assent.'

In what I am about to say, I confine myself to the simple statement of the testimony which they render to the belief of the second and third centuries. For this reason, I exclude from consideration almost, if not altogether, those subsequent to the age of Constantine; and I abstain as far as possible from special details which would lead us astray from the general survey which alone can be given in an address of this kind. I shall thus, I trust, avoid the difficulty, especially to be avoided in this place, of entrenching on theological controversy. I merely state the facts as they occur; and if the results be pleasing or displeasing to the members of this or that school of modern religious opinion, perhaps it will be a sufficient safeguard that they will be almost equally pleasing or displeasing to the members of all such schools equally.

I. I will first ask what, beyond what any books could teach us, we learn of the state of feeling indicated in the very structure of the Catacombs.

The Catacombs are the standing monuments of the Oriental Jewish character even of Western Christianity. The fact that they are the counterparts of the rock-hewn tombs of Palestine, and yet more closely of the Jewish cemeteries in the neighbourhood of Rome, corresponds to the fact that the early Roman Church was not a Latin but an Eastern community, speaking Greek, and following the usages of Syria. And again, the ease with which the Roman Christians had recourse to these cemeteries is an indication of the impartiality of the Roman law, which extended (as Rossi has well pointed out) to this despised sect the same protection in respect to burial, even during the times of persecution, that was accorded to the highest in the land. They thus bear witness to the unconscious fostering care of the Imperial Government over the infant Church. They are thus monuments, not so much of the persecution as of the toleration, which the Christians received at the hands of the Roman Empire.

These two circumstances, confirmed as they are from various quarters, are, as it were, the framework in which the ideas of the Church of the Catacombs are enshrined. They are also circumstances which in the common ecclesiastical histories were as unknown and as much ignored as the existence of the Catacombs themselves.

II. I now proceed to the beliefs themselves, as presented in the pictures or inscriptions, confining myself, as I have said, as much as possible to those which are earliest and most universal. But I will, before entering on these, glance for a moment at those which, though belonging to the latest years of this period—the close of the third century—yet still illustrate the general character even of the earlier. The subjects of these paintings are for the most part taken from the Bible, and are as follows: In the New Testament they are the Adoration of the Magi, the Feeding of the Disciples, Zacchæus in the Sycomore, the Healing of the Paralytic, the Raising of Lazarus, the Washing of Pilate's Hands, Peter's Denial, the Seizure of Peter by the Jews. In the Old Testament they are the Creation, the Sacrifice of Isaac, the Striking of the Rock, Jonah and the Whale, Jonah under the Gourd, Daniel in the Lions' Den, the Three Children in the Fire, Susanna and the Elders.

On this selection I will only make two general remarks. 1. Whilst it does not coincide with the theology and art of the modern Western Church, it coincides to a certain degree with the selection that we find in the Eastern Church. The Raising of Lazarus, for example, fell completely out of the range of the Italian painters and out of the scholastic theology of the Middle Ages; but it may still be traced in the Byzantine traditions as preserved in Russia. In one of the most ancient chapels of the Kremlin there is a representation of the mummy-like form of Lazarus issuing from his tomb, exactly similar to that which appears in the Roman Catacombs. And again: The Three Children, who cease to occupy any important place in the Latin Church, are repeatedly brought forward in the Eastern Church. Three choristers stand in front of the altar at a particular part of the service to represent them, and the only attempt at a mystery or miracle play in the Middle Ages of Russia was the erection of a large wooden platform with the painted appearance of fire under them, on which three actors stood forth and played by gesture and song the part of the Three Children.

2. Secondly, I will observe that the selection of these subjects, whether in the Eastern or in the Western Church, is quite out of proportion to the choice of these same subjects in the books of the time that have come down to us. None of them are conspicuously present in the writers of the three first, or indeed of the sixteen first centuries; and of one of them at least, the arrest of Peter by the Jewish soldiers, it is not too much to say that there is no incident recorded in any extant books to which it can with certainty be applied at all.

I do not dwell on these points to illustrate any contradiction to the existing opinions either of Protestant or Catholic Churches in modern times. The subjects to which these paintings relate for the most part do not involve, even by remote implication, any of these disputed opinions. But they indicate a difference deeper than any mere expression of particular doctrines. They show that the current of early Christian thought ran in an altogether different channel, both from the contemporary writers of the early period, and also both from the paintings and the writings of the later period. In the collection of the works of the Fathers of the second century, there is hardly a single allusion to any one of these topics. Of the paintings of the tenth and eleventh centuries recently discovered in the subterranean church of St. Clement at Rome, not one of all the numerous series is identical with those in the Catacombs. It is a change little noticed in ecclesiastical history, but it was one of momentous consequence to the inner life of the Church.

III. But this peculiarity of the Catacombs thus visible to a certain extent even in the third century, appears still more forcibly when we confine ourselves, according to the plan which I propose in this lecture, to the earliest chambers, and to the most important figures which they contain.

There is one such chamber especially, which, according to the Commendatore De Rossi, is the very earliest that can be found, reaching back to the very beginning of the second century. It is that commonly known as the Catacomb of S. Nereus and Achilleus, otherwise of S. Domitilla.

In this chamber there are three general characteristics:—

1. Everything is cheerful and joyous. This, to a certain degree, pervades all the Catacombs. Although some of them must have been made in times of persecution, yet even in these the nearest approach to such images of distress and suffering is in the figures I have before noticed—(and these are not found in the earliest stage)—the Three Children in the Fire, Daniel in the Lions' Den, and Jonah naked under the Gourd.

But of the mournful emblems which belong to nearly all the later ages of Christianity, almost all are wanting in almost all the Catacombs:

there is neither the cross of the fifth or sixth century, nor the crucifix or the crucifixion of the twelfth or thirteenth, nor the tortures and martyrdoms of the seventeenth, nor the skeletons of the fifteenth, nor the cypresses and death's heads of the eighteenth. There are, instead, wreaths of roses, winged genii, children playing. This is the general ornamentation. I notice the difference—neither to condemn the later, nor to applaud the earlier—but merely to call attention to the fact. It is a variation not noticed in ordinary ecclesiastical history. It is a plainness not visible in the writers of the second and third centuries. But it is there. There are two words used in the very earliest account of the very earliest Christian community to which the English language furnishes no exact equivalent; one is their exulting, bounding gladness (ἀγαλλιάσις); the other, their simplicity and smoothness of feeling, as of a plain without stones, of a field without furrows (ἀφελότης). These two words from the records of the first century (Acts ii. 46) represent to us what appears in the second century in the Roman Catacombs. It may be doubted whether they have ever been equally represented at any subsequent age.

2. Connected with this fact is another. It is the fact that so many of these decorations are taken from heathen sources and copied from heathen paintings. There is Orpheus playing on his harp to the beasts; there is Bacchus as the God of the vintage; there is Psyche, the butterfly of the soul; there is the Jordan as the God of the river. The Classical and the Christian, the Hebrew and the Hellenic, elements had not yet parted. The strict demarcation which the books of the period would imply between the Christian Church and the heathen world had not yet been formed, or was constantly effaced. The Catacombs have more affinity with the chapel of Alexander Severus, which contained Orpheus side by side with Abraham and Christ, than they have with the writings of Tertullian, who spoke of heathen poets only to exult in their future torments, or of Augustine, who regarded this very figure of Orpheus only as a mischievous teacher to be disparaged, not as a type of the union of the two forms of heathen and Christian civilization.

3. We see in the earliest chambers not only the beginning, but in a certain sense the end of early Christian art. By the time we reach the fourth century the figures are misshapen, rude, and stiff, partaking of that decadence which marks the Arch of Constantine, and which is developed into the forms afterwards called Byzantine. But in the second and third centuries, in the Catacombs of S. Domitilla, of S. Prætextatus, and S. Priscilla, there is in the sweetness of the countenance, the depth of the eyes, the grace and majesty of the forms, an inspiration of a higher source, it may be partly from the contact with the still living art of Greece, it may be from the contact with a purer and higher flame of devotion not yet burnt out in fierce controversy.

There is a figure which occurs constantly in the Catacombs, and which in those earliest of all has a peculiar grace of its own—that of the dead person represented in the peculiar position of prayer, which has now entirely ceased in all Christian Churches, but as it may still now and then be seen in Mahometan countries, as it existed universally both in Pagan and perhaps also in Jewish worship in the second century,—the attitude of standing with the hands stretched out to receive the gifts which Heaven would pour into them. Such are the figures of the "Oranti," as they are technically called, in the Catacombs, men or women, according to the sex of the departed. Such also were the holy hands and upturned eyes of the worshippers in the heathen temples of Greece or Rome. The most perfect representation of this in Christian art is, perhaps, that of the departed Christian in the Catacomb of S. Priscilla. The most perfect representation of this in heathen art is, perhaps, that of the bronze figure of an adoring youth, found in the Rhine, of this same period of the Roman Empire, and now in the Museum at Berlin. An animated description which has been given of this in a recent work devoted to Greek art, might, with a few changes of expression, be applied to the painting of the departed Christian in the Catacomb of S. Priscilla. "His eyes and arms are raised to heaven; perfect in humanity beneath the lightsome vault of heaven, he stands and prays,—no adoration with veiled eyes and muttering lips—no prostration, with the putting off of sandals on holy ground—no genuflexion, like the bending of a reed waving with the wind,—but such as Iamus in the mid waves of Alpheius might have prayed when he heard the voice of Phœbus calling to him, and promising to him the twofold gift of prophecy."*

Such is the ideal of the worshipping youth of a Pagan temple of that period—such is the transfigured ideal of the worshipping maiden or matron in the Christian Catacomb. Such has not been the ideal of worship in any later age of the Church.

IV. But the question might here be asked, if these sacred decorations are so like what we find in heathen tombs or houses, how do we know that we are in a Christian burial-place at all? What is the sign that we are here in the chamber of a Christian family? What is the test, what is the watchword, by which these early Christians were known from those who were not Christians?

I have already indicated some of the Biblical subjects which in the third century we should discover; we also know well what we should find in the various later Churches, whether Greek, Latin, Anglican, Lutheran, or Nonconformist. Some distinctive emblems we should find everywhere, either in books, pictures, or statues. But none of these were in the Catacombs even of the third century; and in the Catacombs of the second century, not even those which are found in the third and fourth centuries.

1. What, then, is the test or sign of Christian popular belief that in these earliest representations of Christianity is handed down to us as the most cherished, the all-sufficing token of their creed? It is very

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* Symonds' Greek Poetry.

simple, but it contains a great deal. It is a shepherd in the bloom of youth, with a crook or a shepherd's pipe in one hand, and on his shoulder a lamb, which he carefully carries and holds with the other hand. We see at once who it is; we all know without being told. There are two passages in two of the sacred books, which, whatever may be the critical discussion about their dates, must be inferred from these paintings to have been by that time firmly rooted in the popular belief of the community. One is that from the Third Gospel, which speaks of the shepherd going over the hills of Palestine to seek the sheep that was lost; the other, that from the Fourth Gospel, which says, "I am the Good Shepherd," or, as perhaps we might venture to translate it, "I am the Beautiful Shepherd." This, in that earliest chamber or church of a Christian family of which I am chiefly speaking, is the one sign of Christian life and of Christian belief. But as it is the only, or almost the only sign of Christian belief in this earliest Catacomb, so it continues (with those other pictures of which I have spoken) always the chief, always the prevailing sign, as long as those burial-places were used. Sometimes with one sheep, sometimes with several sheep in various attitudes; some listening to his voice, some turning away. Sometimes in chapels, sometimes on the tombs themselves; sometimes on the tombs of the humblest and poorest; sometimes in the sepulchres of Emperors and Empresses—Galla Placidia and Honorius—but always the chief mark of the Christian life and faith.

There is no allusion to the Good Shepherd (with one exception) in the writers of the second century, and very few in the third; hardly any in Athanasius* or in Jerome. If we come down much later, hardly any in the 'Summa Theologiæ' of Thomas Aquinas, none in the Westminster Confession, none in the Tridentine Catechism, none in the Thirty-nine Articles. The only prominent allusions we find to this figure in the writers of early times are drawn from that same undercurrent of Christian society to which the Catacombs themselves belong. One is the allusion, in an angry complaint of Tertullian, to the chalices used in the Communion, on which the Good Shepherd was a frequent subject; the other is in the one popular book of devotion, the 'Pilgrim's Progress' of the Church of the second century, which was spread far and wide from Italy even to Greece, Egypt and Abyssinia, namely, the once universal, once canonical, once inspired, now forgotten and disparaged, but always curious book, called the "Shepherd of Hermas."

This disproportion between the almost total absence of this figure in the works of the learned, and its predominant prevalence in the thoughts of the people at large in the places where we most surely touch the hearts and thoughts of the first Christians—this gives the answer to the question with which I began this address,—What was the Religion of the first Christians? It was, in one word, the Religion of the Good Shepherd. The kindness, the courage, the grace, the love, the beauty, of the Good Shepherd was to them, if we may so say, Prayer Book and Articles, Creed and Canons, all in one. They looked on that figure, and it conveyed to them all that they wanted. As ages passed on, the Good Shepherd faded away from the mind of the Christian world, and other emblems of the Christian faith have taken his place. Instead of the gracious and gentle Shepherd, there came the Omnipotent Judge, or the Crucified Sufferer, or the Infant in His Mother's arms, or the Master in His Parting Supper, or the figures of innumerable saints and angels, or the elaborate expositions of the various forms of theological controversy.

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* Origen (Hom. v. on Jeremiah iii. 152) has a somewhat detailed reference. His other allusions are of the most perfunctory kind. So also Cyprian (Clem. Alex. Pæd. i. 7, 9; Strom. i. 26), has similar slight references. There is nothing in Irenæus or Justin, and only two passing notices in Tertullian (De Patientiâ, c. 12; De Pudicitiâ, c. 9), besides the condemnatory passage (De Pudicitiâ, c. 13) referred to above.

These changes may have been inevitable. Christianity is too vast and complex to be confined to the expressions of any single age, or of any single nation, and what was suitable for one age may become unsuited for another. Still, it is useful for us to go back to this its earliest form, and ask what must have been the ideas suggested by it.

(*a*) It was an instance of that general connection which I just now noticed between the new Christian belief and the old Pagan world. A figure not unlike the Good Shepherd has from time to time appeared in the Grecian worship. There was the Hermes Kriophorus —Mercury with the ram—as described by Pausanias. There were also the figures of dancing shepherds in the tombs of the Nasones near Rome. In one instance in the Christian Catacombs, the Good Shepherd appears surrounded by the Three Graces.* In the tomb of Galla Placidia, He might well be the youthful Apollo playing with his pipes to the flocks of Admetus. There had not yet sprung up the fear of taking as the chief symbol of Christianity an idea or a figure which would be equally acknowledged by Pagans.

(*b*) It represents to us the joyful, cheerful side of Christianity, of which we spoke before. Look at that beautiful, graceful figure, bounding down as if from his native hills, with the happy sheep nestling on his shoulder, with the pastoral pipes in his hand, blooming in immortal youth. It is the exact representation of the Italian shepherd as we constantly encounter him on the Sabine hills at this day, holding the stray lamb on his shoulders, with a strong hand grasping the twisted legs as they hang on his breast. Just such a one appears on a fresco in the so-called house of Livia, on the Palatine. That is the primitive conception of the Founder of Christianity. It is the very reverse of that desponding, foreboding, wailing cry that we have often heard in later days, as if His religion were going to die out of the world; as if He were some dethroned prince, whose cause was to be cherished only by the reactionary, losing, vanquished parties of the world or Church. The popular conception of Him in the early Church was of the strong, the joyous youth, of eternal growth, of immortal grace.

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* De Rossi, ii. 358.

(*c*) It represents to us an aspect of the only Christian belief that has not been common in later times, but of which we find occasional traces even in the writings of these earlier centuries, namely, that the first object of the Christian community was not to repel, but to include—not to condemn, but to save. In some of the paintings of the Good Shepherd, this aspect of the subject is emphasized by representing the creature on his shoulder to be not a lamb, but a kid; not a sheep, but a goat.

It is this which provokes the indignant remonstrance of Tertullian in the only passage of the Fathers which contains a distinct reference to the popular representation of the Good Shepherd; and it is on this unchristian protest that the most classic poet and the keenest critic of our day—I need hardly name Matthew Arnold—founds one of his most touching poems.

> "'He saves the sheep—the goats he doth not save;'
> So spoke the fierce Tertullian.
> But she sigh'd—
> The infant Church; of love she felt the tide
> Stream on her from her Lord's yet recent grave;
> And then she smil'd, and in the catacomb
> With eye suffused, but heart inspired true,
> She her Good Shepherd's hasty image drew,
> And on his shoulder not a lamb, but kid."

(*d*) It represents to us the extreme simplicity of this early belief. It seems as if that key-note was then struck in the popular Christianity of those first ages, which has in its best aspects made it the religion of little children and guileless peasants, and also of child-like philosophers and patriarchal sages.

There is here nothing strange, difficult, mysterious. But there was enough to satisfy the early Christian, to nerve the suffering martyr, to console the mourner. When Bosio, the first explorer of the Catacombs in the seventeenth century, opened the tomb of which I have been speaking, he was disappointed when he found only the Good Shepherd, and went on to other later chambers and chapels, where there were other more varied pictures, and other more complicated emblems. He did not know that this one, which he despised for its simplicity, was the most interesting of all, because the earliest of all.

It is possible that others, like Bosio, have gone farther and fared worse in their dissatisfaction at so simple a representation. It is certain, as I have said, that, till quite modern times,* the Good Shepherd, and the ideas which the figure suggested, had become as strange and rare as the doctrines of later times would have seemed strange to the dwellers in the Catacombs.

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* It occurs in the pictures of the French Huguenots of the 17th century, preserved in the Protestant Library in the Place Vendôme.

2. The Good Shepherd, however, is not the only figure which pervades the tomb of Domitilla. There is another which also, in like manner, predominates elsewhere.

It is a vine painted on the roof and on the walls, with its branches spreading and twisting themselves in every direction, loaded with clusters of grapes, and seeming to reach over the whole chamber. And sometimes this figure of the Vine is the only sign of Christian belief. In the tomb of Constantia, the sister of the Emperor Constantine, even the Good Shepherd does not appear; the only decorations that are carved on her coffin and painted on the walls are children gathering the vintage, plucking the grapes, carrying baskets of grapes on their heads, dancing on the grapes to press out the wine. The period in which the figure of the Vine appears is more restricted than that in which the figure of the Shepherd appears. But taking, again, the tomb of Domitilla as our main example, it is undeniable that if the chief thought of the early Christians was the Good Shepherd, the second was the Vine and the Vintage.

What is the meaning of this? There are three ideas which we may suppose to have been represented.

(*a*) The first is that which I have noticed before—the joyous and festive character of the primitive Christian faith. In Eastern countries the vintage is the great holiday of the year. In the Jewish Church there was no festival so gay and so free as the Feast of Tabernacles, when they gathered the fruit of the vineyard, and enjoyed themselves in their green bowers or tabernacles.

I remember hearing Lord Macaulay describe, with all his force of language and variety of illustration, how natural and beautiful was the origin of the heathen legend, which represented the victorious march of Dionysus, the inventor of the vine, and how everyone must have been entranced at the coming in of their new guest—the arrival of the life-giving grape—scattering joy and merriment wherever he came. Something of this kind seems to have been the sentiment of the early Christian community. No doubt the monastic and the Puritan element existed amongst them in germ, and showed itself in the writings even of the second and third centuries; but it is evident from these paintings that it occupied a very subordinate place in the popular mind of the early Roman Christians.

It may be that the hideous associations which northern drunkenness has imported into these festive emblems have rendered impossible to modern times a symbol which in earlier days and in southern countries was still permissible. It may be that after the disappointments, controversies, persecutions, mistakes, scandals, follies of Christendom for the last seventeen centuries, it is impossible to imagine that buoyant heart, that hopeful spirit, which then was easy and natural. Not the less, however, is it instructive for us to see the joyous gaiety, the innocent Bacchanalia, with which our first fathers started in the dawn of that journey which has since been so often overcast.

(*b*) There was, however, perhaps a deeper thought in this figure. When we see the vine, with its purple clusters spreading itself over the roof of the chamber, it is difficult not to feel that the early Christians had before their minds the recollection of the Parable of "The Vine and the Branches." When we see the juice of the grapes streaming from the feet of those who tread the winepress,—the figures, frequent in the Jewish Scriptures, represented in colossal form over the portal of the Jewish Temple, carved still on Jewish sepulchres—culminating to the Christian mind in that sacred apologue. It was the account which they gave to themselves and to others of the benefits of their new religion. What they valued, what they felt, was a new moral influence, a new life stealing through their veins, a new health imparted to their frames, a new courage breathing in their faces, like wine to a weary labourer, like sap in the hundred branches of a spreading tree, like juice in the thousand clusters of a spreading vine.

Where this life was, there was the sign of their religion. By what special channel it came, whether through books or treatises, whether through bishops or presbyters, whether through this doctrine or that, this the paintings in the Catacombs—at least in the earliest Catacombs—do not tell us. All that we see is the Good Shepherd on one side, and the spreading Vine and joyous vintage on the other side. It was an influence as subtle, as persuasive, as difficult to fix into one uniform groove, as what we call the influence of love, or marriage, or law, or civilization.

(*c*) The figure of the Vine, as seen in the Catacombs, suggests perhaps one other idea — the idea of what was then meant by Christian unity. The branches of the Vine are infinite; no other plant throws out so many ramifications, which twist and clasp and turn and hang and creep and rise and fall in so many festoons and roots and clusters and branches, over trees and houses; sometimes high, sometimes low, sometimes graceful, sometimes deformed, sometimes straight, sometimes crooked. But in all there is the same life-giving juice, the same delicious fragrance. That is the figure of the Vine as we see it in the tomb of S. Domitilla. It is a likeness—whether intended or not—of the variety and unity of Christian goodness.

V. There is one other subject on which we should naturally expect in these Catacombs to learn some tidings of the belief of the early Christians, and that is concerning the future life and the departed. This we gather partly from their paintings, but chiefly from their epitaphs.

In these representations there are three such characteristics, agreeing with what we have already noticed.

1. First, there is the same simplicity. If for a moment we look at the paintings on this subject, in what form are the souls of the dead presented to us? Almost always in the form of little birds; sometimes with bright, gay plumage—peacocks, pheasants, and the like; more often as doves. There was here, no doubt, the child-like thought, that

the soul of man is like a bird of passage, which nestles here in the outward frame of flesh for a time, and then flies away beyond the sea to some brighter, warmer home. There was the thought that the Christian soul ought to be like "the birds of the air," according to the Gospel phrase, without anxiety or solicitude. There was the thought also that each Christian soul is, like the dove, a messenger of peace, is part of the heavenly brood which flies upwards towards that Spirit of which it is the emanation and the likeness.

And when we come to the epitaphs of the ancient dead, we find still the same simple feeling. There is no long description; till the third century, not even the date; no formal profession of belief; no catalogue either of merits or demerits; but, generally speaking, one short word to tell of the tender sentiment of natural affection: "My most sweet child"; "My most sweet wife"; "My most dear husband"; "My innocent dove"; "My well-deserving father or mother"; "Such and such an one lived together, without any complaint or quarrel, without taking or giving offence."

Amongst all the epitaphs and monuments of Westminster Abbey, there is one, and one only, which reminds us of the Catacombs. It is that of a little Yorkshire girl, who lies in the cloisters, and who died in the midst of the troubles which preceded the Revolution of 1688. There are just the dates, and the name of her brother, whom the parents had lost a short time before, and who is buried in St. Helen's Church, in York; and all that they say of her is, "*Jane Lister, dear child.*" That is exactly like the Catacombs; that is the perpetual sympathy of human nature. In these words the whole Christian world, from the nineteenth century to the first, "is kin."

And if, in the outpouring of this natural affection, the survivors from time to time refuse to lose sight of the dead in the other world, it is still to be remarked that the communion with them rests on this family bond, and on none other. There is one touching devotional poem of modern date, which seems to me more than any other to recall the peculiar feeling of the early Catacombs in this respect. It is that of the Russian poet Chamiakoff, on visiting the nursery of his dead children:—

> "Time was when I loved at still midnight to come,
> My children, to see you asleep in your room;
> Dear children, at that same still midnight do ye,
> As I once prayed for you, now in turn pray for me." *

2. But besides these expressions of natural affection, there are two expressions of religious devotion which constantly occur. The first is repeated almost in every epitaph—"*In peace.*" It is the phrase which the early Christians took from the Jews. In the Jewish Catacombs it is found in the Hebrew word—"*Shalom.*" As the expressions which I have just quoted indicate the link between the belief of the early Christians and the natural feelings of the human heart, so

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* I have ventured to borrow the translation of the Rev. William Palmer.

does this indicate the link between their belief and that of ancient Judaism. But its earnest reiteration gives a special force to it. It conveys their assurance that whatever else was the other world, it was at least a world of rest. The wars, the jealousies, the jars, the contentions, the misapprehensions, the disputes of the Roman Empire and of the Christian Church, would there at last be finished. "Sleep" —"repose"—is the word—vague, indefinite, but sufficient—for the condition of their departed friends. The burial-places of the world henceforth became what they were first called in the Catacombs—or at least first * called on an extensive scale—cemeteries—that is, "sleeping-places."

3. There is one other word which occurs frequently after the mention of "peace," and that is, "*Live in God*," or "thou shalt live in God," or "mayest thou live in God," or "thou livest in God." This is the yet farther step from simple innocence, from Oriental resignation. That is the early Christians' expression of the ground of their belief in immortality. We might perhaps have expected some more precise allusion to the sacred name by which they were especially called, or to some of those Gospel stories of which we do, at least in the third century, find representations in their pictures. But in these epitaphs it is not so. They were content in the written expression of their belief to repose their hopes in the highest name of all.

These simple words—"*Vive in Deo*" and "*Vivas in Deo*"—describe what to them was the object and the ground of their existence for the first three centuries. They last appear in the year 330, and after that appear no more again till quite modern times, in curious imitation of them, as for example in the beautiful epitaph on the late lamented Duke John of Tortlonia, in the Church of S. John Lateran. As a general rule, nowhere now, either in Roman Catholic or Protestant churches, do we ever see these once universal expressions of the ancient hope. They have been superseded by more definite, more detailed, more positive statements. Perhaps if they were now used they would be thought Deistic, or Theistic, or Pantheistic, or Atheistic. But when we reflect upon them, they run very deep down into the heart both of philosophy and of Christianity. They express the hope that because the Supreme Good lives for ever, all that is good and true will live for ever also. They express the hope that because the Universal Father lives for ever, we can safely trust into His loving hands the souls of those whom we have loved, and whom He, we cannot help believing, has loved also.

Perhaps the more we think of this ancient style of epitaph, we shall find that it is not the less true because now it is never written; not the less consoling because it is so ancient; not the less comprehensive because it is so simple, so short, and so childlike.

VI. Let me briefly sum up what I have said on these representations of the early Christian belief.

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* Mommsen says that the words *κοιμητήριον*, *accubitorium*, are not exclusively Christian.

1. They differ widely in proportion, in selection, and in character, from the representations of belief which we find in the contemporaneous Christian authors, and thus give us a striking example of the divergence which often exists between the actual living, popular belief, and that which we find in books. They differ also in the same respects, though even more widely, from the forms adopted, not only by ourselves, but by the whole of Christendom, for nearly fifteen hundred years: they show what it is never without interest to observe, the immense divergence in outward expression of belief between those ages and our own. The forms which we use were unused by them, and the forms which they used, for the most part are unused by us.

2. The substance of the faith which these forms expressed is such as, when it is put before us, we at once recognize to be true.

It might sometimes be worth while to ask whether what are called attacks or defences of our religion are directed in the slightest degree for or against the ideas which, as we have seen, constitute the chief materials of the faith and life of the early Christians. In a well-known German book, entitled 'The Old and New Belief,' there is an elaborate attack on what the writer calls "the Old Belief." Of the various articles of that "old belief" which he enumerates, hardly one appears conspicuously in the Catacombs. Of the special forms of belief which appear in the Catacombs, hardly one is mentioned in the catalogue of doctrines so vehemently assailed in that work. The belief of the Catacombs, as a general rule, is not that which is either defended by modern theologians * or attacked by modern sceptics.

3. When we reflect that these same ideas which form the all-sufficing creed of the early Church, are not openly disputed by any Church or sect in Christendom, it may be worth while to ask whether, after all, there is anything very absurd in supposing that all Christians have something in common with each other. The pictures of the Good Shepherd and of the Vine, the devotional language of the epitaphs—I know not whether they would be called sectarian or unsectarian, denominational or undenominational—but they have not been watchwords of parties; no public meetings have been held for defending or abolishing them, no persecutions or prosecutions have been set on foot to put them down or to set them up; and yet it is certain that, by the early Christians, they were not thought vague, fleeting, unsubstantial, colourless, but were the food of their daily lives—their hope under the severest trials—the very dogma of dogmas, if we choose so to call them—the very life of their life.

[A. P. S.]

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* In the Lateran Museum are two or three compartments of epitaphs classed under the head of "illustrations of *dogmas*." But the only instance that can be possibly construed as relating to a dogma controverted by any Christian Church is the doubtful abbreviation *In D. Christo;* which may be either Deo or Divo.

## GENERAL MONTHLY MEETING,

Monday, June 1, 1874.

George Busk, Esq. F.R.S. Treasurer and Vice-President,
in the Chair.

The Rev. James Langton Clarke, M.A.
Jamieson Elles, Esq.
John C. Gostling, Esq.
Edward Langley, Esq.
The Rev. John Lane Oldham, M.A. F.G.S.
Edward Power, Esq. F.R.G.S.

were *elected* Members of the Royal Institution.

The special thanks of the Members were given to the Lord Clermont for his valuable present of 'The Life, Works, and Family History of Sir John Fortescue, Knt. Chief Justice and Lord Chancellor to King Henry the Sixth.' (Edited by Lord Clermont, and printed for private distribution.) 2 vols. 4to. 1869.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

*Asiatic Society of Bengal*—Journal, 1873, Part 1, No. 4; Part 2, Nos. 1, 2. 8vo.
Proceedings, 1874, No. 1. 8vo.
*Astronomical Society, Royal*—Monthly Notices, Vol. XXXIV. No. 6. 8vo. 1874.
*British Architects, Royal Institute of*—Sessional Papers, 1873–4, No. 10. 4to.
*Chemical Society*—Journal for May, 1874. 8vo.
*Clermont, The Lord (the Editor)*—Sir John Fortescue: His Life, Works, and Family History. (Printed for private circulation.) 2 vols. 4to. 1869.
*Duckworth, Dyce, M.D. M.R.I. (the Author)*—Observations on Sleeplessness. (K 100) 8vo. 1874.
*Editors*—Astronomical Register for May, 1874. 8vo.
Athenæum for May, 1874. 4to.
Chemical News for May, 1874. 4to.
Engineer for May, 1874. fol.
Food Journal for May, 1874. 4to.
Journal for Applied Science for May, 1874. fol.
Journal of Gas-Lighting for May, 1874. 4to.
Nature for May, 1874. 4to.
Nautical Magazine for May, 1874. 8vo.
Pharmaceutical Journal for May, 1874. 8vo.
Telegraph Journal for May, 1874. 8vo.
*Ellis, A. J. Esq. F.R.S. M.R.I. (the Author)*—Algebra identified with Geometry. (K 100) 8vo. 1874.

*Franklin Institute*—Journal, No. 580. 8vo. 1874.
*Geographical Society, Royal*—Journal for 1873. 8vo. 1874.
*Geological Institute, Imperial, Vienna*—Jahrbuch, 1874. No. 1. 8vo. Verhandlungen, 1874, Nos. 1–6. 8vo.
*Iron and Steel Institute*—Journal, 1873, Vol. I. No. 2. 8vo. 1874.
*Liverpool Polytechnic Society*—Journal, April, May, 1874. 8vo.
*Mechanical Engineers' Institution, Birmingham*—Proceedings. Jan. 1874 8vo.
*Preussische Akademie der Wissenschaften*—Monatsberichte: Marz, 1874. 8vo.
*Puyals de la Bastida, Don Vicente (the Author)*—Teoria de los Numeros. (O 16) 16to. 1874.
*Royal Society of London*—Proceedings, No. 152. 8vo. 1874.
*Shipley, Rev. Orbey, M.A. (the Editor)*—The Church and the World. Essays on Questions of the Day, in 1866 and 1867. By Various Writers. 2 vols. 8vo. 1867–8.
*Tyndall, Professor, D.C.L. LL.D. F.R.S. &c. (the Author)*—The Forms of Water. 4th Edition. 16mo. 1874.
*United Service Institution, Royal*—Journal: Appendix to Vol. XVII. 8vo. 1874.
*Wild, M. H. (the Director)*—Annalen des Physikalischen Central-Observatoriums, Jahrgang, 1872. 4to. 1873.
*Woodworth, J. M. M.D. (the Author)*—Report on Marine Hospital Service, United States. 8vo. 1873.

## WEEKLY EVENING MEETING.

Friday, June 5, 1874.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Professor J. Burdon Sanderson, M.D. F.R.S.

### *On the Mechanism of the Leaf of Dionæa Muscipula, and on the Electrical Phenomena which accompany its Contraction.*

After introductory observations on the application of the experimental method in the study of physiology, the speaker proceeded to divide his subject into two parts. In the first he proposed to describe and illustrate the general characters of the plant; in the second, to discuss the property of contractility possessed by the leaf, and to demonstrate its complete similarity to that of the contractile tissues of animals, by a series of experiments.

### Part I.—*Description of the leaf of Dionæa, and of the mode in which it catches and digests insects.*

The general aspect of the plant was illustrated by a large drawing. It belongs to the order Droseraceæ, a very small but very natural order, *i.e.* one of which each species exhibits the signs of original relationship very distinctly. It is one in respect of which it is also evident enough that the relationship is rather one of descent than of community of parentage, for the common peculiarities by which the plants which form it are distinguished from other plants, are not possessed by all of them in anything like equal degree; so that here, as elsewhere among plants and animals, we have indications that the more perfect forms stand to the less perfect rather in the relation of descendants than in that of cousins. The most striking of these peculiarities is one of function, not of structure. It lies in this, that all the members of the order exhibit in one form or other adaptedness to one end—the catching and digesting of insects. These animal endowments, which have for some years engaged the attention of our great naturalist, are possessed by each species in a degree which in the main corresponds to the completeness of the plant's development in regard of form and structure; so that each advance from a less to a greater perfection in these respects, is accompanied by an improvement in the adaptedness of the mechanism by which insects are caught and digested.

Dionæa stands alone at the head of its order; for in all respects, both as regards structure and as regards the perfection of its insect-catching mechanism, it is superior to all the rest. Like the Droseras of our English bogs, it requires only soft, damp moss to root in, and its roots are correspondingly simple. Its flowers possess little beauty, and for us, very little interest. It is on the leaves that our attention must be fixed.

The leaf is of a very peculiar form. Its blade consists of two nearly semicircular lobes, joined together along their straight borders by a strong midrib. On to this midrib the two lobes, when the leaf is open, are set nearly at right angles to each other. The curved outer edge of each lobe is strengthened by a hem, from which some twenty spikes spring on either side, which are directed towards the concavity of the leaf. The under surface of the leaf is bright green, smooth and glistening, and is marked with parallel streaks which run outwards from the midrib; but the upper surface is pink, owing this colour to its being beset with little red bosses, too small to be seen by the naked eye, which are called glands. In addition to these glands, there are on each lobe three delicate hair-like spines or filaments, which are always arranged as if at the corners of a triangular space, about the middle of the upper surface. The petiole, or leaf-stalk, is of the shape of the handle of a teaspoon, the only difference being that its upper surface is channelled along the middle, instead of being flat. By its broad end it is united to the leaf by a narrow isthmus, of about half a line in breadth.

Whenever an insect, or any other object, touches however lightly any of the six filaments above mentioned, the lobes close sharply upon it, just as a rat-trap closes on the animal caught; the closure is at first, however, only partial. When the leaf is thus half closed, it is easy to see what is the significance of the two sets of prongs along its edges. They are observed to be set alternately along the borders of the two lobes, so as to fit into each other as the teeth of a rat-trap do. The reason why the leaf does not at once close firmly on its prey—at first sight not very apparent—Mr. Darwin has been able to explain by observing what happens when an insect finds itself caught and attempts to escape. If it is small, it easily makes its way through the grating formed by the crossing of the teeth, and in this case, the leaf expands again very gradually, and is then ready for another victim. If it is large, all its efforts to regain its liberty are futile. Repelled by its prison bars, it is driven back upon the sensitive filaments, which project into the interior of its cell, and again irritates them. By doing so it occasions a second and more vigorous contraction of the lobes. The result is that the creature is this time not only captured, but crushed, to be eventually digested.

The leaf having thus closed, remains for a week or more in the same state. At first the two lobes are so flatly pressed against each other that the insect between them forms a bulging projection on either side. But soon a quantity of juice is secreted by the internal surface of the leaf, which as it collects distends the space occupied by the insect, which after a time becomes diffluent, and is eventually absorbed. This process of gradual liquefaction and absorption Mr. Darwin has shown by numerous experiments to be of the same nature as that by which food is digested in the stomach of the higher animals. (This was illustrated by experiments.)

Part II.—*Experimental demonstration that the contractility of Dionæa corresponds in all important respects with that of the contractile tissues of animals.*

The speaker began by pointing out that in all the sciences of observation the process by which we learn to understand new facts is that of comparing them with those previously known, and that accordingly he would make all his experiments comparative. He would first make experiments to illustrate some of the leading properties of contractile tissues, particularly the very remarkable relation between contraction and the electrical changes which accompany it, and then repeat the same experiments with the Dionæa leaf; whence it would be seen that the correspondence between the two series of phenomena is so complete that it is impossible to doubt of their essential identity. But before proceeding, it would be advisable to state the grounds of comparison generally, including some particulars, which, although there was not time to illustrate them experimentally, were of no less importance than the others.

The best example of contractility in the higher animals is that of voluntary muscle—the best, not merely because it exhibits the property of contracting in answer to excitation in perfection, but because it has been so perfectly investigated by physicists, that the conditions under which the property manifests itself are very exactly known.

The following are some of the most important facts as regards muscle:—

(1) In contracting it changes its shape, but not its bulk. Most muscles are spindle-shaped. In contracting, a muscle becomes shorter, but at the same time thicker in corresponding proportion, so that although its ends are nearer to each other, its bulk remains unaltered.

(2) This change of shape of the whole muscle depends on an exactly similar change of shape of every particle of which it is composed. This might be inferred from the consideration that a muscle is not an apparatus made up of parts differing from each other in structure, but a mass of substance equally instinct with life in every part. We know it to be the case by direct observation, for if we observe living muscle in the act of contraction under the microscope (as can easily be done in the muscles of insects), we see that each minutest fibre participates in the change of form.

The same holds good as regards the plant. The agent in the contraction is the protoplasm, by which the cells forming the contractile organs are lined. This, as Mr. Darwin has shown, exhibits under the influence of irritation changes of form which, although they do not resemble those which we see in contracting muscle, are no less distinct and characteristic.

(3) Muscle, like every other living tissue, is the seat, so long as it lives, of chemical changes, which, if the tissue is mature, consist entirely in the disintegration of chemical compounds and the dissipation of the force stored up in these compounds, in the form of heat or

some other kind of motion. This happens when the muscle is at rest, but much more actively when it is contracting, in which condition it not only produces more heat than it produces at other times, but also may do,—and, under ordinary circumstances does—mechanical work; these effects of contraction of muscle are, of course, dependent in quantity on the chemical disintegration which goes on in its interior.

(4) Finally, muscle so long as it is in the living state is electromotive. This property it probably possesses in common with other living tissues, for it is very likely that every vital act is connected with electrical change in the living part. But in muscle, as well as other irritable and contractile tissues in animals, the manifestation of electromotive force is inseparably connected with the special function of the tissue, *i. e.* with contraction, the connection being of such a nature that the electromotive force expresses, not the work actually done at any given moment, but the capacity for work. Thus, so long as the muscle lives, its electromotive force is found to be on the whole proportional to its vigour. As it gradually loses its vitality, its power of contracting and its electromotive force disappear *pari passu. When it contracts, the manifestation of electromotive force diminishes in proportion to the degree of contraction, and ceases when the contraction is complete.*

These propositions were illustrated by experiments, showing:—

1. That muscle when stimulated contracts, changing its form and doing work.
2. That the electromotive force of muscle, as shown galvanoscopically, declines when it contracts.
3. That this happens also when muscle contracts automatically.
4. That the same result is observed when the leaf of Dionæa is excited either mechanically or electrically.
5. That in Dionæa the period which elapses between the stimulation and its effect (period of latent stimulation) is much longer than in muscle; and that
6. Each stimulation is succeeded by a period of exhaustion, during which, although its electromotive force is restored, it is not excitable.

In commenting on the last experiment the speaker cautioned the physiological student against what he conceived to be a likely misapprehension of the facts. When muscle or Dionæa contracts, electromotive force disappears and work is done; but there is no reason for supposing that there is any conversion of the one effect into the other, or that the source of the force exercised by the muscle or the leaf in contracting is electrical.

[The plants used in the experiments and exhibited at the lecture, had been obtained through the kindness of the Director of the Royal Gardens, Kew. They had been cultivated expressly for the occasion, and were in an unusually advanced growth for the season.]

## GENERAL MONTHLY MEETING,

Monday, July 6, 1874.

THE EARL OF ROSSE, D.C.L. F.R.S. Vice-President,
in the Chair.

William Walter Brocklehurst, Esq.
Bernard Dietz, Esq.
William Thomas Hustwitt, Esq.
Frank Philip Leon, Esq.
Charles D. F. Phillips, M.D.
Winwood Reade, Esq.

were *elected* Members of the Royal Institution.

The Secretary reported that LADY FELLOWS, the widow of SIR CHARLES FELLOWS, who was long a Member and frequently a Manager of the Royal Institution, had bequeathed to the Institution her Drawings of Sir Charles's celebrated Collection of Watches, bequeathed by him to the British Museum.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

*American Academy of Arts and Sciences*—Proceedings, Vol. VIII. Nos. 64–84. 8vo. 1873.
Complete Works of Count Rumford. Vols. II. III. 8vo. 1873–4.
*American Philosophical Society*—Transactions, New Series, Vol. XV. Part 1. 4to. 1873.
Proceedings for 1873. 8vo.
*Asiatic Society, Royal.*—Journal, Vol. VII. Part 1. 8vo. 1874.
*Astronomical Society, Royal*—Monthly Notices, Vol. XXXIV. No. 7. 8vo. 1874.
*British Architects, Royal Institute of*—Sessional Papers, 1873–4, No. 11. 4to.
*Chemical Society*—Journal for June, 1874. 8vo.
*Editors*—American Journal of Science for June, 1874. 8vo.
Astronomical Register for June, 1874. 8vo.
Athenæum for June, 1874. 4to.
Chemical News for June, 1874. 4to.
Engineer for June, 1874. fol.
Food Journal for June, 1874. 4to.
Journal for Applied Science for June, 1874. fol.
Journal of Gas-Lighting for June, 1874. 4to.
Nature for June, 1874. 4to.
Nautical Magazine for June, 1874. 8vo.
Pharmaceutical Journal for June, 1874. 8vo.
Telegraph Journal for June, 1874. 8vo.

*Edmonds, R. Esq. (the Author)*—On the Name "Britain" and the Phœnicians. (Devon Assoc. Trans. 1871.) 8vo. 1871.

Oscillations in Lake Ontario, &c. ('Quart. Journal of Science,' 1874.) 8vo.

*Franklin Institute*—Journal, No. 581. 8vo. 1874.

*Geographical Society, Royal*—Proceedings, Vol. XVIII. No. 3. 8vo. 1874.

*Geological Society*—Quarterly Journal, No. 118. 8vo. 1874.

*Gladstone, Dr. J. H. F.R.S. M.R.I.*—L'Aéronaute: Bulletin Mensuel International de la Navigation Aérienne: dirigé par Dr. A. H. de Villeneuve. 8vo. 1873–4.

*Kershaw, S. W. M.A. (the Author)*—Lambeth Palace Library and its Kentish Memoranda (with his "Art Treasures"). 8vo. 1874.

*Leeds Philosophical Society.*—Report, 1872–73. 8vo. 1874.

*Manchester Geological Society*—Transactions, Vol. XIII. Part 4. 8vo. 1874.

*Medical and Chirurgical Society, Royal*—Proceedings, Part 39. 8vo. 1874.

*Meteorological Office*—Quarterly Weather Report, 1873. Part 2. 4to.

*Meteorological Society*—Quarterly Journal, New Series, Nos. 9, 10. 8vo. 1874.

*Photographic Society*—Journal, No. 251. 8vo. 1874.

*Smith, H. A. Esq. (the Author)*—Chemistry of the Sulphuric Acid Manufacture. 16to. 1873.

*St. Petersburg Imperial Academy of Sciences*—Mémoires, VIIe Série, Tome XIX. Nos. 8–10. Tome XX. Tome XXI. Nos. 1–5. 4to. 1872–3.

Bulletins, Tome XVIII. Nos. 3–5. Tome XIX. Nos. 1–3. 4to. 1872–3.

Repertorium für Meteorologie, Band III. 4to. 1874.

*Symons, G. J. Esq. (the Author)*—Symons' Monthly Meteorological Magazine, June, 1874. 8vo.

*Tozer, Serjeant J. LL.D. M.A. M.R.I. (the Author)*—On Identity of Method in the Search for Truth. Jurisprudence. (K 100) 8vo. 1874.

*United States Naval Observatory*—Washington Astronomical and Meteorological Observations for 1871. 4to. 1873.

*United Service Institution, Royal*—Journal, No. 76. 8vo. 1874.

*Victoria Institute*—Journal, No. 29. 8vo. 1874.

*Wisconsin State Agricultural Society*—Transactions, Vols. X. XI. 8vo. 1872.

*Zoological Society*—Transactions, Vol. VIII. Nos. 7, 8. 4to. 1874.

Proceedings, 1873, No. 3. 1874, No. 1. 8vo.

# Royal Institution of Great Britain.

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## GENERAL MONTHLY MEETING,

Monday, November 2, 1874.

**Warren De la Rue**, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

Charles Frederick Moxon, Esq.
Sir David Lionel Salomons, Bart.
Henry Arthur Smith, Esq.

were *elected* Members of the Royal Institution.

The Managers reported the resignation of **Dr. Wm. Rutherford**, Esq. F.R.S.E. the Fullerian Professor of Physiology. A new Professor will be appointed on April 5, 1875.

The following letter from the **Hon. Justice Grove** to the Secretary was read:—

"Chettle House, Blandford,
"*October* 15, 1874.

"Dear Spottiswoode,

"Some time ago I noticed, that among the busts in the anteroom of the Royal Institution, there was not one of **Barlow**, who in my judgment by his assiduity and disinterested services gave a great impetus to the Institution, and for the long time he was Secretary was untiring in his zeal. I have had a marble bust executed by **Mr. Durham**, A.R.A., which I am told is very good. I saw the cast and thought it excellent, and for a posthumous work done from photographs a fair likeness.

"If the Managers will do me the honour to accept it for the Institution it will give me much pleasure. It is now at **Mr. Durham's**, 21, Devonshire Street, and can be removed when desired.

"Yours very truly,
"W. R. Grove."

**Resolved**—That the best thanks of this Meeting be presented to the **Hon. Justice Grove** for his munificent gift to the Royal Institution of a marble bust of the late **Rev. John Barlow**, for so many years its Honorary Secretary, which they receive with peculiar gratification, as evincing the interest taken in the Institution by so distinguished a worker in science as **Sir William Robert Grove**, and as a well-deserved memorial of **Mr. Barlow's** long and devoted services to the Institution.

**The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same.**

FROM

*The French Government*—Documents Inédits sur l'Histoire de France: Mélanges Historiques: Nouvelle Série, Tome I. 4to. 1873.
Mandements et Actes Divers de Charles V. 1364–80. 4to. 1874.
*The India Office*—Synopsis of the Great Trigonometrical Survey of India, Vol. I. 4to. 1874.
*The British Museum Trustees*—Catalogue of Birds, Vol. I. Plates. 8vo. 1874.
Guide to Exhibition Rooms. 8vo. 1874.
Catalogue of Roman Medallions. By H. A. Grüber. 8vo. 1874.
Guide to Egyptian Galleries. 12mo. 1874.
Hand List of Seals, &c. 8vo. 1873.
*Academy of Natural Sciences, Philadelphia*—Proceedings, 1873. 8vo.
*Actuaries, Institute of*—Journal, No. 95. 8vo. 1874.
*Antiquaries, Society of*—Proceedings, Vol. VI. Nos. 2, 3. 8vo. 1874.
*Asiatic Society of Bengal*—Journal, 1873, Part 2, No. 4; 1874, Part 1, Nos. 1, 2; Part 2, No. 1. 8vo.
Proceedings, 1874, Nos. 2, 3, 4, 5, 6. 8vo.
*Astronomical Society, Royal*—Monthly Notices, Vol. XXXIV. No. 8. 8vo. 1874.
*Bavarian Academy of Science*—Abhandlungen. Band XI. Abth. 3. 4to. 1874.
Sitzungsberichte, 1873. Heft 3. 1874. Hefte 1–3. 8vo.
Rede und Denkschriften über Liebig. 4to. 1874.
Verzeichniss von 5563 Telescopischen Sternen. 8vo. 1874.
*Belgique, Académie Royale de*—Bulletin, Tomes XXXV. XXXVI. 8vo. 1873.
Almanach, 1874. 16to.
*Bertin, M. Emile (the Author)*—Notes on Waves and Rolling. ('Naval Science,' July, 1874.) 8vo.
*Boston Society of Natural History*—Memoirs, 4 Parts. 4to. 1873–4.
Proceedings, 3 Parts. 8vo. 1873.
*British Architects, Royal Institute of*—Sessional Papers, 1873–4, Nos. 12, 13. 4to.
*British Association for the Advancement of Science*—Report of the 42nd Meeting: at Bradford, 1873. 8vo. 1874.
*Brussels Royal Observatory*—Annales Météorologiques, 1872, 1873. 4to. 1874.
*Chemical Society*—Journal for July–Oct. 1874. 8vo.
*Comitato Geologico d'Italia*—Bollettini, 1874. Nos. 1–6. 8vo. 1874.
*Devonshire Association for the Advancement of Science*—Report and Transactions for 1873. Vol. VI. Part 2. 8vo. 1874.
*Dulau, Messrs. (the Publishers)*—W. D. Cooley: Dr. Livingstone and the Royal Geographical Society. (K 100) 8vo. 1874.
*Editors*—American Journal of Science for July to Sept. 1874. 8vo.
Astronomical Register for July to Oct. 1874. 8vo.
Athenæum for July to Oct. 1874. 4to.
Chemical News for July to Oct. 1874. 4to.
Engineer for July to Oct. 1874. fol.
Journal for Applied Science for July to Oct. 1874. fol.
Nature for July to Oct. 1874. 4to.
Nautical Magazine for July to Oct. 1874. 8vo.
Pharmaceutical Journal for July to Oct. 1874. 8vo.
Quarterly Journal of Science for July and Oct. 1874.
Telegraph Journal for July to Oct. 1874. 8vo.
*Fellows, Lady (Widow of Sir Charles Fellows, M.R.I.)*—Drawings by herself of Thirty-nine of the Watches collected by Sir Charles Fellows, now in the British Museum. (*A legacy.*)
*Franklin Institute*—Journal, Nos. 582–585. 8vo. 1874.
*Geographical Society, Royal*—Proceedings, Vol. XVIII. No. 4. 8vo. 1874.

*Geological Institute, Imperial, Vienna*—Jahrbuch, 1874. No. 2. 8vo.
Verhandlungen, 1874, Nos. 7–11. 8vo.
Abhandlungen, Vol. VII. Nos. 1, 2. fol. 1874.
*Geological Society*—Quarterly Journal, No. 119. 8vo. 1874.
*Harlem, Société des Sciences*—Verhandelingen. 3de Verz. Deel II. No. 1. 4to. 1874.
Archives Néerlandaises, Tome IX. 1, 2, 3. 8vo. 1872–4.
*Iron and Steel Institute*—Journal, 1874. 8vo. 1874.
*Kirkpatrick, H. C. Esq. (the Author)*—Report of the Action for Libel brought by the Rev. R. O'Keeffe, P.P., against Cardinal Cullen. 8vo. 1874.
*Leeds Philosophical Society*—Report, 1873. 8vo. 1874.
*Levitloux, M. H. (the Author)*—Philosophie de la Nature. 3e éd. Paris, 1874. 8vo.
*Linnean Society*—Journal, Nos. 76, 77. 8vo. 1874.
Proceedings, 1873–4. 8vo.
Transactions, Vol. XXVIII. Part 4; Vol. XXIX. Part 2; Vol. XXX. Part 1. 4to. 1874.
*Lunacy Commissioners*—Report for 1873. 8vo.
*Manchester Geological Society*—Transactions, Vol. XIII. Part 5. 8vo. 1874.
*Manchester Philosophical Society*—Proceedings, Vol. XIV. Nos. 1, 2. 8vo. 1874.
*Mechanical Engineers' Institution, Birmingham*—Proceedings, April, 1874. 8vo.
*Medical and Chirurgical Society, Royal*—Transactions, Vol. LVII. 8vo. 1874.
*Meteorological Office*—Meteorological Committee of the Royal Society: Report for 1873. 8vo. 1874.
Report on Weather Telegraphy and Storm Signals. 8vo. 1874.
Monthly Charts of Meteorological Data, with Remarks. 2 vol. fol. and 4to. 1874.
*Meteorological Society*—Quarterly Journal, New Series, No. 11. 8vo. 1874.
*Musée Teyler*—Archives, Vol. III. Fasc. 4. 8vo. 1874.
*Penzance Public Library*—Catalogue. 8vo. 1874.
*Photographic Society*—Journal, No. 252. 8vo. 1874.
*Porter, T. C. Esq. and J. M. Coulter, Esq. (the Authors)*—Synopsis of the Flora of Colorado. 8vo. 1874.
*Preussische Akademie der Wissenschaften*—Monatsberichte, April–Aug. 1874. 8vo.
*Roberts, W. C.*—Fourth Annual Report of the Deputy-Master of the Mint, 1873. 8vo. 1874.
*Roma, Accademia dei Lincei*—Atti, Anno XXVI. Sess. 2. 4to. 1874.
*Royal College of Surgeons*—Calendar. 8vo. 1874.
*Royal Society of London*—Proceedings, Nos. 153, 154, 155. 8vo. 1874.
Philosophical Transactions for 1874, Part 1. 4to. 1874.
*St. Bartholomew's Hospital*—Statistical Tables. 1873. 8vo.
*Smithsonian Institution*—Smithsonian Report, 1872. 8vo.
*Statistical Society*—Journal, Vol. XXXVII. Parts 2, 3. 8vo. 1874.
*Symons, G. J. Esq. (the Author)*—Symons' Monthly Meteorological Magazine, July, Aug. Sept. Oct. 1874. 8vo.
*Tremaux, P. (the Author)*—Principe Universel du Mouvement. (O 16) 16to. 1874.
*Twining, Thomas, Esq. M.R.I. (the Author)*—Technical Training. 8vo. 1874.
*Tyndall, Professor, LL.D. D.C.L. F.R.S. (the Author)*—On the Atmosphere as a Vehicle of Sound. ('Phil. Trans.' 1874.)
Heat a Mode of Motion. (In Hungarian.) 8vo. Buda-Pesth. 1874.
Fragmente aus den Naturwissenschaften: Uebersetzt von A. H. Mit Vorwort und Zusätzen von H. Helmholtz. 8vo. Braunschweig. 1874.
*United Service Institution, Royal*—Journal, No. 77. 8vo. 1874.
*Vereins zur Beförderung des Gewerbfleisses in Preussen*—Verhandlungen, Mai-Juni. 1873. 4to.
*Victoria Institute*—Journal, No. 30. 8vo. 1874.
*Walford, Weston S. Esq. F.S.A. M.R.I.*—Papworth's Ordinary of British Armorials. Parts 22, 23, &c. (Completion of the work.) 8vo. 1874.
*Zoological Society*—Transactions, Vol. VIII. No. 9. 4to. 1874.
Proceedings, 1874, Nos. 2, 3. 8vo.

## GENERAL MONTHLY MEETING,

### Monday, December 7, 1874.

Admiral Sir Henry J. Codrington, K.C.B. Manager, in the Chair.

The Hon. Mrs. Francis Wm. Buxton,
William Henry Domville, Esq.
George Sampson, Esq.

were *elected* Members of the Royal Institution.

The following Lecture Arrangements were announced:

John Hall Gladstone, Esq. Ph.D. F.R.S. Fullerian Professor of Chemistry, R.I.—Six Lectures on the Voltaic Battery; on December 29, 31, 1874; January 2, 5, 7, 9, 1875.

### Before Easter, 1875.

E. Ray Lankester, Esq. M.A.—Six Lectures on the Pedigree of the Animal Kingdom; on Tuesdays, January 12 to February 16.

Alfred H. Garrod, Esq.—Four Lectures on Animal Locomotion; including Locomotion on Land, in the Air, and in Water; on Tuesdays, February 23, March 2, 9, 16.

Professor P. M. Duncan, F.R.S.—Three Lectures on the Grander Phænomena of Physical Geography; on Thursdays, January 14 to 28.

Professor Tyndall, D.C.L. LL.D. F.R.S.—Seven Lectures on Electricity; on Thursdays, February 4 to March 18.

Edward Dannreuther, Esq.—Two Lectures on Mozart and Beethoven (with Pianoforte Illustrations); on Saturdays, January 16 and 23.

J. T. Wood, Esq.—Four Lectures on the Discovery of the Temple of Diana and other Results of the Government Excavations at Ephesus; on Saturdays, January 30, February 6, 13, and 20.

Professor W. K. Clifford, M.A. F.R.S.—Four Lectures on the General Features of the History of Science; on Saturdays, February 27 to March 20.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same.

From

*The Lords of the Admiralty*—Nautical Almanac, 1878. 8vo. 1874.

*Actuaries, Institute of*—Journal, No. 96. 8vo. 1874.

*Agricultural Society of England, Royal*—Journal, Second Series, Vol. X. Part 2. 8vo. 1874.

*Alcock, T. St. L. Col. M.R.I.*—The Relative Power of Nations, Part II. (K 100) 8vo. 1874.

*American Academy of Arts and Sciences*—Proceedings, Vol. IX. 8vo. 1874.

*American Philosophical Society*—Proceedings, No. 92. 8vo. 1874.

*British Architects, Royal Institute of*—Sessional Papers, 1874–5, No. 1. 4to.

*Carpenter, W. B. M.D. F.R.S. &c.*—Further Inquiries on Oceanic Circulation. (Royal Geograph. Soc. Proceedings, Vol. XVIII. 1874.)

*Chemical Society*—Journal for Nov. 1874. 8vo.
*Civil Engineers' Institution*—Minutes of Proceedings, Vols. XXXVII. XXXVIII. 8vo. 1874.
*Cornwall Polytechnic Society, Royal*—Forty-first Annual Report, 1873. 8vo. 1874.
*Editors*—American Journal of Science for Oct. 1874. 8vo.
  Astronomical Register for Nov. 1874. 8vo.
  Athenæum for Nov. 1874. 4to.
  Chemical News for Nov. 1874. 4to.
  Engineer for Nov. 1874. fol.
  Journal for Applied Science for Nov. 1874. fol.
  Nature for Nov. 1874. 4to.
  Nautical Magazine for Nov. 1874. 8vo.
  Pharmaceutical Journal for Nov. 1874. 8vo.
  Quarterly Journal of Science for Nov. 1874.
  Telegraph Journal for Nov. 1874. 8vo.
*Franklin Institute*—Journal, Nos. 584, 585. 8vo. 1874.
*Geological Society*—Quarterly Journal, No. 120. 8vo. 1874.
*Jervis, Wm. Esq. (the Author)*—Cenni Geologici sulle Montagne poste in Prossimità al Giacimento di Antracite di Demonte. (L 16) 8vo. 1873.
*Linnean Society*—Journal, No. 58. 8vo. 1874.
*Manchester Philosophical Society*—Proceedings, Vol. XIV. No. 3. 8vo. 1873.
*Meteorological Office*—Quarterly Weather Report, 1871, Part 4; 1873, Part 3. 4to.
*Meteorological Society*—Quarterly Journal, New Series, No. 12. 8vo. 1874.
*Odling, Professor, M.A. F.R.S. M.R.I. (the Author)*—Abstracts of Lectures delivered at the Royal Institution. 8vo. 1868–73.
*Photographic Society*—Journal, No. 253. 8vo. 1874.
*Rae, John, M.D. M.R.I. (the Author)*—On some Physical Properties of Ice. ('Phil. Mag.' July, 1874.) 8vo.
*St. Bartholomew's Hospital*—Statistical Tables: 1874. 8vo.
*Smithsonian Institution*—Temperature Chart of the United States.
*Spottiswoode, Wm. Esq. LL.D. F.R.S. Sec. R.I. &c. (the Author)*—Polarization of Light. ('Nature' Series.) 16to. 1874.
*Statistical Society*—Journal, Index to Vols. XXVI.–XXXV. 8vo. 1874.
*Symons, G. J. Esq. (the Author)*—Symons' Monthly Meteorological Magazine, Nov. 1874. 8vo.
*Trafford, F. W. C. Esq. (the Author)*—Amphiorama, ou la Vue des Montagnes de la Spezia. (K 100) 8vo. 1874.
*United Service Institution, Royal*—Journal, No. 78. 8vo. 1874.
*Victoria Institute*—Journal, No. 31. 8vo. 1874.
*Wilson, W. E. Esq. M.R.I.*—Photograph of the Moon. Sept. 29, 1874.

## WEEKLY EVENING MEETING,

Friday, January 15, 1875.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Professor Tyndall, D.C.L. LL.D. F.R.S. M.R.I.

### *On Acoustic Reversibility.*

On the 21st and 22nd of June, 1822, a Commission appointed by the Bureau of Longitudes of France executed a celebrated series of experiments on the velocity of sound. Two stations had been chosen, the one at Villejuif, the other at Montlhéry, both lying south of Paris, and 11·6 miles distant from each other. Prony, Mathieu, and Arago were the observers at Villejuif, while Humboldt, Bouvard, and Gay-Lussac were at Montlhéry. Guns, charged sometimes with 2 lb. and sometimes with 3 lb. of powder, were fired at both stations, and the velocity was deduced from the interval between the appearance of the flash and the arrival of the sound.

On this memorable occasion an observation was made which, as far as I know, has remained a scientific enigma to the present hour. It was noticed that while every report of the cannon fired at Montlhéry was heard with the greatest distinctness at Villejuif, by far the greater number of the reports from Villejuif failed to reach Montlhéry. Had wind existed, and had it blown from Montlhéry to Villejuif, it would have been recognized as the cause of the observed difference; but the air at the time was calm, the slight motion of translation actually existing being from Villejuif towards Montlhéry, or against the direction in which the sound was best heard.

So marked was the difference in transmissive power between the two directions, that on the 22nd of June, while every shot fired at Montlhéry was heard "à merveille" at Villejuif, but one shot out of twelve fired at Villejuif was heard, and that feebly, at the other station.

With the caution which characterized him on other occasions, and which has been referred to admiringly by Faraday,* Arago made no attempt to explain this anomaly. His words are: "Quant aux différences si remarquables d'intensité que le bruit du canon a toujours présentés suivant qu'il se propageaient du nord au sud entre Villejuif et Montlhéry, ou du sud au nord entre cette seconde station et la première; nous ne chercherons pas aujourd'hui à l'expliquer, parceque nous ne pourrions offrir au lecteur que des conjectures denuées de preuves."*

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* 'Researches in Chemistry and Physics,' p. 484.

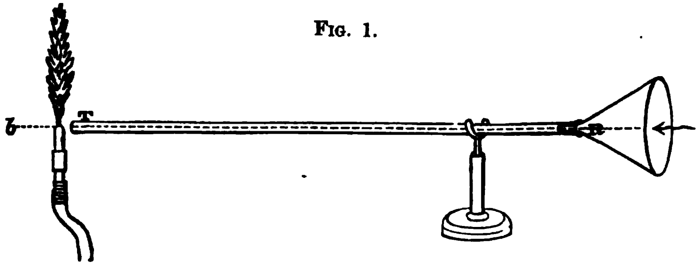
I have tried, after much perplexity of thought, to bring this subject within the range of experiment, and have now to submit a possible solution of the enigma. The first step was to ascertain whether the sensitive flame referred to in my recent paper in the 'Philosophical Transactions' could be safely employed in experiments on the mutual reversibility of a source of sound and an object on which the sound impinges. Now the sensitive flame usually employed by me measures from 18 to 24 inches in height, while the reed employed as a source of sound is less than a square quarter of an inch in area. If, therefore, the whole flame, or the pipe which fed it, were sensitive to sonorous vibrations, strict experiments on reversibility with the reed and flame might be difficult, if not impossible. Hence my desire to learn whether the seat of sensitiveness was so localized in the flame as to render the contemplated interchange of flame and reed permissible.

The flame being placed behind a cardboard screen, the shank of a funnel passed through a hole in the cardboard was directed upon the middle of the flame. The sound-waves issuing from the vibrating reed placed within the funnel produced no sensible effect upon the flame. Shifting the funnel so as to direct its shank upon the root of the flame, the action was violent.

To augment the precision of the experiment, the funnel was connected with a glass tube three feet long and half an inch in diameter, the object being to weaken by distance the effect of the waves diffracted round the edge of the funnel, and to permit those only which passed through the glass tube to act upon the flame.

Presenting the end of the tube to the orifice of the burner (*b*, Fig. 1), or the orifice to the end of the tube, the flame was violently agitated by the sounding-reed, R. On shifting the tube, or the burner, so as to concentrate the sound on a portion of the flame about half an inch above the orifice, the action was *nil*. Concentrating the sound

FIG. 1.



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* 'Connaissance des Temps,' 1825, p. 370.

upon the burner itself about half an inch below its orifice, there was no action.

These experiments demonstrate the localization of "the seat of sensitiveness," and they prove the flame to be an appropriate instrument for the contemplated experiments on reversibility.

The experiments proceeded thus: The sensitive flame being placed close behind a screen of cardboard 18 inches high by 12 inches wide, a vibrating reed, standing at the same height as the root of the flame, was placed at a distance of six feet on the other side of the screen. The sound of the reed, in this position, produced a strong agitation of the flame.

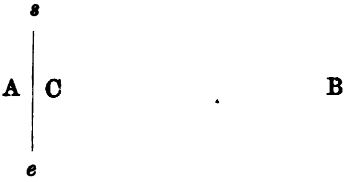
The whole upper half of the flame was here visible from the reed; hence the necessity of the foregoing experiments to prove the action of the sound on the upper portion of the flame to be *nil*, and that the waves had really to bend round the edge of the screen so as to reach the seat of sensitiveness in the neighbourhood of the orifice of the burner.

The positions of the flame and reed were reversed, the latter being now close behind the screen, and the former at a distance of six feet from it. The sonorous vibrations were without sensible action upon the flame.

The experiment was repeated and varied in many ways. Screens of various sizes were employed; and instead of reversing the positions of the flame and reed, the screen was moved so as to bring, in some experiments the flame, and in other experiments the reed, close behind it. Care was also taken that no reflected sound from the walls or ceiling of the laboratory, or from the body of the experimenter, should have anything to do with the effect. In all cases it was shown that the sound was effective when the reed was at a distance from the screen and the flame close behind it; while the action was insensible when these positions were reversed.

Thus, let *s e*, Fig. 2, be a vertical section of the screen. When the reed was at A and the flame at B there was no action; when the reed was at B and the flame at A the action was decided. It may be added that the vibrations communicated to the screen itself, and from it to the air beyond it, were without effect; for when the reed, which at B is effectual, was shifted to C, where its action on the screen was greatly augmented, it ceased to have any action on the flame at A.

Fig. 2.



We are now, I think, prepared to consider the failure of reversibility in the larger experiments of 1822. Happily an incidental observation of great significance comes here to our aid. It was observed and recorded at the time, that while the reports of the guns at Villejuif were without echoes, a roll of echoes, lasting from 20 to 25 seconds, accompanied every shot at Montlhéry, being heard by the observers there. Arago, the writer of the report, referred these echoes to reflection from the clouds, an explanation which I think we are entitled to regard as problematical. The report says that "tous les coups tirés à Montlhéry y étaient *accompagnés* d'un roulement semblable à celui du tonnerre." I have italicized a very significant word—a word which fairly applies to our experiments on gun-sounds at the South Foreland, where there was no sensible solution of continuity between explosion and echo, but which could hardly apply to echoes coming from the clouds. For supposing the cloud to have been only a mile distant, the sound and its echo would have been separated by an interval of nearly ten seconds. But there is no mention of any interval; and had such existed, surely the word "followed," instead of "accompanied," would have been the one employed. The echoes, moreover, appear to have been *continuous*, while the clouds observed seem to have been *separate*. "Ces phénomènes," says Arago, "n'ont jamais eu lieu qu'au moment de l'apparition de quelques nuages." But from separate clouds a continuous roll of echoes could hardly come. When to this is added the experimental fact that clouds far denser than any ever formed in the atmosphere are demonstrably incapable of sensibly reflecting sound, while cloudless air, which Arago pronounced echoless, has been proved capable of powerfully reflecting it, I think we have strong reason to question the hypothesis of the French philosopher.

And considering the hundreds of shots fired at the South Foreland, with the attention specially directed to the aerial echoes, when no single case occurred in which echoes of measurable duration did not accompany the report of the gun, I think Arago's statement that at Villejuif no echoes were heard, when the sky was clear, must simply mean that they vanished with great rapidity. Unless the attention were specially directed to the point, a slight prolongation of the cannon-sound might well escape observation; and it would be all the more likely to do so if the echoes were so loud and prompt as to form apparently part and parcel of the direct sound.

I should be very loth to transgress here the limits of fair criticism, or to throw doubt, without good reason, on the recorded observations of illustrious men. Still, taking into account what has been just stated, and remembering that the minds of Arago and his colleagues were occupied by a totally different problem—that the echoes were an incident rather than an object of observation—I think we may justly consider the sound which he called "instantaneous" to have been one whose aerial echoes did not differentiate themselves from the direct sound by any noticeable fall of intensity, and which died rapidly into silence.

Turning now to the observations at Montlhéry, we are struck by the extraordinary duration of the echoes heard at that station. At the South Foreland the charge habitually fired was equal to the largest of those employed by the French philosophers; but on no occasion did the gun-sounds produce echoes approaching to 20 or 25 seconds' duration. The time rarely reached half this amount. Even the syren-echoes, which were more remarkable and more long-continued than those of the gun, never reached the duration of the Montlhéry echoes. The nearest approach to it was on the 17th of October, 1873, when the syren-echoes required 15 seconds to subside into silence.

On this same day, moreover (and this is a point of marked significance), the transmitted sound reached its maximum range, the gun-sounds being heard at the Quenocs buoy, 16½ nautical miles distant from the South Foreland. I have stated in another place that the duration of the air-echoes indicates "the atmospheric depths" from which they come.* An optical analogy may help us here. Let light fall upon chalk, the light is wholly scattered by the superficial particles; let the chalk be powdered and mixed with water, light reaches the observer from a far greater depth of the turbid liquid. The solid chalk typifies the action of exceedingly dense acoustic clouds; the chalk and water that of clouds of moderate density. In the one case we have echoes of short, in the other echoes of long duration. These considerations prepare us for the inference that Montlhéry, on the occasion referred to, must have been surrounded by a highly diacoustic atmosphere; while the shortness of the echoes at Villejuif shows the atmosphere surrounding that station to have been acoustically opaque.

Have we any clue to the cause of the opacity? I think we have. Villejuif is close to Paris, and over it, with the observed light wind, was slowly wafted the air from the city. Thousands of chimneys to windward of Villejuif were discharging their heated currents; so that an atmosphere non-homogeneous in a high degree must have surrounded that station. At no great height in the atmosphere the equilibrium of temperature would be established. This non-homogeneous air, surrounding Villejuif, is experimentally typified by our screen with the source of sound close behind it, the upper edge of the screen representing the place where equilibrium of temperature was established in the atmosphere above the station. In virtue of its proximity to the screen, the echoes from our sounding-reed would, in the case here supposed, so blend with the direct sound as to be practically indistinguishable from it, as the echoes at Villejuif followed the direct sound so hotly, and vanished so rapidly, that they escaped observation. And as our sensitive flame, at a distance, failed to be affected by the sounding body placed close behind the cardboard screen, so, I take it, did the observers at Montlhéry fail to hear the sounds of the Villejuif gun.

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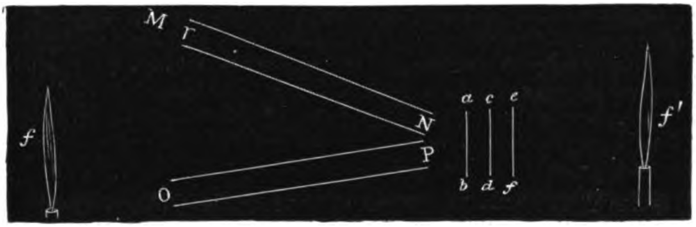
* 'Phil. Trans.' 1874, pt. i. p. 202.

Something further may be done towards the experimental elucidation of this subject. The facility with which sounds pass through textile fabrics has been already illustrated,* a layer of cambric or calico, or even of thick flannel or baize, being found competent to intercept but a small fraction of the sound from a vibrating reed. Such a layer of calico may be taken to represent a layer of air differentiated from its neighbours by temperature or moisture; while a succession of such sheets of calico may be taken to represent successive layers of non-homogeneous air.

Two tin tubes (M N and O P, Fig. 3) with open ends were placed so as to form an acute angle with each other. At the end of one was the vibrating reed *r*; opposite the end of the other, and in the prolongation of P O, the sensitive flame *f*, a second sensitive flame (*f'*) being placed in the continuation of the axis of M N. On sounding the reed, the direct sound through M N agitated the flame *f'*. Introducing the square of calico *a b* at the proper angle, a slight decrease of the action on *f'* was noticed, and the feeble echo from *a b* produced a barely perceptible agitation of the flame *f*. Adding another square, *c d*, the sound transmitted by *a b* impinged on *c d*; it was partially echoed, returned through *a b*, passed along P O, and still further agitated the flame *f*. Adding a third square, *e f*, the reflected sound was still further augmented, every accession to the echo being accompanied by a corresponding withdrawal of the vibrations from *f'* and a consequent stilling of that flame.

Fig. 3.



With thinner calico or cambric, it would require a greater number of layers to intercept the entire sound; hence with such cambric we should have echoes returned from a greater distance, and therefore of greater duration. Eight layers of the calico employed in these experiments, stretched on a wire frame and placed close together as a kind of pad, may be taken to represent a dense acoustic cloud. Such a pad, placed at the proper angle beyond N, cuts off the sound, which in its absence reaches *f'*, to such an extent that the flame *f'*, when not too sensitive, is thereby stilled, while *f* is far more powerfully agitated than by the reflection from a single layer. With the source of sound close at hand, the echoes from such a pad would be of insensible duration. Thus close at hand do I suppose the acoustic clouds surrounding Villejuif to have been, a similar shortness of echo being the consequence.*

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* 'Phil. Trans.' 1874, pt. i. p. 208.

A further step is here taken in the illustration of the analogy between light and sound. Our pad acts chiefly by internal reflection. The sound from the reed is a composite one, made up of partial sounds differing in pitch. If these sounds be ejected from the pad in their pristine proportions, the pad is acoustically *white;* if they return with their proportions altered, the pad is acoustically *coloured.*

In these experiments my assistant, Mr. Cottrell, has rendered me material assistance.

[J. T.]

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* Since this was written I have sent the sound through fifteen layers of calico, and echoed it back through the same layers, in strength sufficient to agitate the flame. Thirty layers were here crossed by the sound. The sound was subsequently found able to penetrate two hundred layers of cotton net; a single layer of wetted calico being competent to stop it.

## WEEKLY EVENING MEETING,

Friday, January 22, 1875.

Sir Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

Sir John Lubbock, Bart. M.P. F.R.S. M.R.I.

VICE-CHANCELLOR OF THE UNIVERSITY OF LONDON,

### *On the Relations of English Wild Flowers to Insects.*

The speaker followed out in general the line adopted in his just published work on 'British Flowers considered in Relation to Insects.' He commenced with a short history of the subject, referring in terms of warm praise especially to the labours of Sprengel, Darwin, Hildebrand, Delpino, and Muller. Sprengel pointed out the close relations which existed between flowers and insects, and the service rendered by the latter in transferring the pollen from the stamen to the pistil; but Darwin was the first to perceive that the importance of this consisted, not merely in the transference of the pollen from one organ to another, but from one plant to another. Everyone indeed knows how important flowers are to insects; that bees, butterflies, &c., derive the main part of their nourishment from flowers, but comparatively few are aware, on the other hand, how much the flowers themselves are dependent on insects. Yet it is not too much to say that if flowers are very useful to insects, insects, on the other hand, are in many cases absolutely necessary to flowers: that if insects have been in some respects modified and adapted with a view to the acquirement of honey and pollen, flowers, on the other hand, owe their scent and colours, nay, their very existence, in their present form, to insects. Thus, the lines and bands by which so many flowers are ornamented have reference to the position of the honey; and it may be observed that these honey-guides are absent in night flowers, where they of course would not show, and would therefore be useless; as for instance, in *Lychnis vespertina*, or *Silene nutans*. Night flowers, moreover, are generally pale; for instance, *Lychnis vespertina* is white, while *Lychnis diurna*, which flowers by day, is red.

This transference of the pollen takes place in almost all species; but, while in most flowers it is effected by insects, in some cases it is simply caused by the wind. Wind-fertilized flowers, however, have no colour, no scent, and no honey. The self-fertilization of flowers is provided against in three principal ways. Sometimes the stamens and pistil are situated in different flowers; sometimes they come to maturity at different times; sometimes they are so arranged that the pollen from the stamens could only reach the pistil with greater or less difficulty. In those plants in which the stamens and pistil are not mature simultaneously, the pistil in some cases ripens first, as in the aristolochia and arum; but in the great majority the stamens ripen before the pistil.

In illustration of the great influence which insects exercise over plants, the speaker then called attention to those cases in which within a single genus we meet with species having large, and others with small flowers, as, for instance, in *epilobium* and *geranium;* and pointed out that the large flowers were those most dependent upon insects.

Of course these conclusions implied that insects were capable of distinguishing colours, but the speaker then proceeded to mention some experiments which he had made, and which seemed to prove directly that this was the case. For instance, he placed some honey on a slip of glass, and put the glass on blue paper, and when the bee had made several journeys, and thus become accustomed to the blue colour, he placed some honey in the same manner on orange paper. Then during one of the absences of the bee he transposed the two colours, leaving the honey itself in the same place as before. The bee returned as usual to the place where she had been accustomed to find the honey; but though it was still there she did not alight, but paused for a moment, and then dashed straight to the blue paper. No one, he said who saw this bee at that moment could have had the slightest doubt of her power of distinguishing blue from orange. He mentioned one other experiment. Having accustomed a bee to come to honey on blue paper, he ranged other supplies of honey on paper of other colours, yellow, orange, red, green, black and white. Then he continually transposed the coloured paper, leaving the honey on the same spots, but the bee always flew to the blue paper wherever it might be.

Sir John then proceeded to describe a number of common flowers, and to show how beautifully they are adapted to secure and profit by the visits of insects, taking as illustrations the berberry, heath, dead-nettle, salvia, sweet pea, daisy, *cypripedium*, &c.

He then passed on to those cases in which cross-fertilization is secured by the relative position of the stamens and pistil; especially in the cases of *primula* and *lythrum*. He referred very briefly to the modifications undergone by bees in order to adapt them to flowers; and after mentioning the well-known cases of the sleep of flowers, as being possibly connected with their relation to insects, and recording some observations on this part of the subject,—for instance, by keeping flowers in the dark he induced them to sleep all day, while in other cases he kept them awake all night by means of artificial light—in conclusion he remarked that the observations commenced by Sprengel have shown that insects, and especially bees, have an importance in relation to flowers which had been previously unsuspected.

To them we owe the beauties of our gardens, the sweetness of our fields. To them flowers are indebted for their scent and colour, nay, their very existence in its present form. Not only have the brilliant colours, the sweet scent, and the honey of flowers been gradually developed by the unconscious agency of insects, but the very arrangement of the colours—the circular bands and radiating lines, the form, size, and position of the petals, the arrangement of the stamens and pistil—all have reference to the visits of insects, and are disposed in such a manner as to ensure the great object which these visits are destined to effect. For it is obvious that any blossom differing from the form and size best adapted to secure the due transference of the pollen would be less likely to be fertilized than others; while, on the other hand, those which were rich in honey, which were the sweetest and the most conspicuous, would most attract the attention and secure the visits of insects; and thus, just as our gardeners, by selecting seed from the most beautiful varieties, have done so much to adorn our gardens, so have insects, by fertilizing the largest and most brilliant flowers, unconsciously, but not less effectually, contributed to the beauty of our woods and fields.

[J. L.]

## WEEKLY EVENING MEETING,

Friday, January 29, 1875.

The Duke of Northumberland, D.C.L. President, in the Chair.

Professor Huxley, LL.D. Sec.R.S.

### *On the Recent Work of the 'Challenger' Expedition, and its Bearing on Geological Problems.*

The speaker began by commenting on the sailing of H.M.S. 'Challenger' in December, 1872, the nature of her mission, and the completeness and perfection of her equipment, under the auspices of the late First Lord of the Admiralty, Mr. Goschen.

After referring to the course laid down for the 'Challenger,' and her expected return in the spring of 1876, he said that her work is rather more than half done, and that important results had been already obtained, some of which would be especially dealt with in his discourse; but it was necessary, first, to give a sketch of the state of our knowledge on the subject in 1872, in order to do justice to the 'Challenger's' work, without over-estimating its value.

The first successful attempt to ascertain the nature of the bottom of the sea, and of the limit of life at depths greater than five or six hundred feet, was made by Sir John Ross in 1818, who, by the help of a very ingenious machine of his own contrivance, brought up several pounds of mud from 1050 fathoms, or 6300 feet, or about a mile and a quarter, in Baffin's Bay, 72° 30′ N., 77° 15′ W. The nature of what was brought up was described by Ross and by Sabine, who accompanied him. It was a fine greenish mud, but no accurate determination of the nature of this mud was made. Ehrenberg, in 1853, examined the surface scum and the mud obtained by Penny in 73° and 74° N., and found it to consist of

*Diatoms* (vegetable), which live at the surface.
*Radiolaria* (animal), which live at the surface.
*Sponge spicules* (animal), which live at the bottom.

In 1854, Bailey determined the nature of the mud procured by Brooke in 900–2700 fathoms (5400–16,200 feet), in the Sea of Kamschatka. There were absolutely no calcareous organisms in the mud; it was purely silicious. These and other observations in the north circumpolar area tend to establish the existence of a circumpolar zone of silicious deposit in the Arctic regions, within the parallel of 55° N.—a *North Polar silicious cap.*

The speaker next alluded to the despatch of the Antarctic expedition under Sir James Ross, in 1839, and the awakening of attention to the importance of the work done by minute organisms by Ehrenberg, in 1836 and 1838. Ehrenberg discovered that organisms, whose skeletons resemble those which occur in cretaceous and tertiary rocks, and sometimes constitute their whole mass, are still living. The observations of Sir James Ross and Dr. Hooker, at two distant points of the Antarctic zone, proved the existence of a *South Polar silicious cap.*

The speaker then noticed the discovery of the nature of the sediment forming the bottom of the deep sea in the *intermediate zone* (about 110° of latitude), of *calcareo-silicious deposit.* This dates back to 1853, and is due to Ehrenberg's examination of the soundings brought up by Berryman from 2000 fathoms (12,000 feet), between Newfoundland and the Azores. They consisted of silicious *Diatoms*, *Radiolaria*, and *Sponge spicules*, as in the Arctic and Antarctic seas: but the great mass of the deposit was formed of calcareous *Foraminifera.* On these observations Ehrenberg based the conclusion then enunciated, that "chalk is nothing but a heap of dead foraminiferal skeletons."

Professor Huxley then alluded to the confirmation of these results by Bailey, himself, and others, and to the discovery of the extension of a similar deposit over the South Atlantic and into the Indian Ocean; and the further confirmation on a great scale by the 'Challenger.'

Having mentioned Ehrenberg's opinion, that *Globigerinæ*, &c., live at the bottom, and the evidence adduced by Dr. Wallich in confirmation of that view, he referred to Major Owen's results, and to the 'Challenger's' confirmation of the affirmative proposition that they certainly live at the top; he added, that the negative proposition that they do not also live at the bottom is left open. The fact that animals which do live at the bottom feed on the Globigerinæ is probably of no great weight.

The speaker next adverted to the *Greensand deposits* of various geological ages, the nature of which was discovered by Ehrenberg in 1854, and to the nearly contemporaneous discovery of recent greensand (100–300 fathoms) by Pourtales (1854), and the investigation of its origin by Bailey in 1856; while Parker and Jones subsequently ascertained the formation of greensand to be going on in the Australian seas. The 'Challenger's' discovery of greensand at the bottom of the Agulhas current (200–300 fathoms) was then mentioned. Thus far the value of the work of the 'Challenger' lies in extending and confirming conclusions already more or less established. The next matter is especially her own.

The barrenness of the deeper parts of the Mediterranean, and the existence of a fine clay there, was already known; but no one suspected the existence of extensive deposits of barren clay in the middle of the great calcareo-silicious zone in open ocean.

The dredgings of the 'Challenger' have recently shown that certain deep oceanic valleys contain thick deposits of finely-divided red clay, composed of a silicate of alumina and peroxide of iron. Thus, between Teneriffe and S. Thomas a great valley was found at a depth of about 18,000 feet, the bottom of which was covered with this red material. The origin of such deposits is suggested by the results of some experiments by Mr. Buchanan, the chemist to the 'Challenger,' who, by treating the Globigerina-marl with dilute acids, obtained about 1 or 2 per cent. of insoluble residuum, which strikingly resembled the clay in question. Hence the conclusion drawn by Professor Wyville Thomson, that the great deep-seated deposits of red clay actually represent the remains of marine organisms, of which myriads must have suffered decomposition to furnish these vast accumulations of their *débris*. If this conclusion be correct, not only silicious and calcareous, but argillaceous deposits may be formed by the long-continued action of vital agencies. And as, by well-known processes of metamorphosis, such silicious, calcareous, and argillaceous deposits may be converted into all the chief kinds of rock which constitute the earth's crust, it will follow that rocks of almost any mineralogical composition may be directly or indirectly generated by the action of living organisms. Having discussed various explanations and dismissed them as all at present unsatisfactory, the speaker said that the certainties are:

1. That, beyond certain depths, the calcareous organisms which must fall over the area covered by the ocean disappear, and their place is taken by fine red clay.
2. That when the ordinary Globigerina ooze has its calcareous matter removed, a residuum of fine red clay remains.

But standing firmly on the basis of bare facts, as they are now ascertained, it is permissible to use these facts as a basis for deductions. Suppose the globe of the earth to be covered with water uniformly to a depth of two thousand fathoms (twelve thousand feet, or about two miles), the merely tidal and current movements which would then exist in the water would be insufficient to cause any important amount of degradation of the solid crust, and there would be no sedimentary deposits worth speaking of.

Now let the lowest forms of vegetable and animal life—*Diatoms, Radiolaria, Foraminifera*, and *Sponges*—be introduced, and the result would be the formation of the silicious pole caps, and of the calcareo-silicious intermediate zone; and these might accumulate until they formed beds of rottenstone and of chalk, of many thousand feet in thickness.

By well-known metamorphic processes the rottenstone might be converted into a dense opaline rock, or perhaps into quartzite; while the chalk might be converted into crystalline limestone; and in each of these all trace of its origin might disappear.

let parts of the bottom in the calcareo-silicious area be raised to within 1000 feet, and others depressed to 18,000 feet. Judging by our present knowledge, the former might be replaced by greensand, and the latter by red clay. The greensand might be metamorphosed into endless varieties of those minerals into which silica, alumina, iron, and potash enter in diverse proportions; the clay into shales, schists, slates, and even into gneiss and granite.

And thus our imaginary world would be covered with more or less extensive patches of all the most important rocks which enter into the composition of the globe, every particle of which would at one time have formed a part of a living organism; though in some, no trace of this primitive composition would remain, and though it is quite possible that all traces of organic life might be obliterated in every one of them.

Application of these results was then made to the reconciliation of an apparent discrepancy between biological theory and geological fact.

1. There is conclusive evidence that the present species of animals and plants have arisen by gradual modification of pre-existing species.
2. There is conclusive evidence that, even geologically speaking, the process has been very slow, and has continued over a much longer period than that which corresponds with the fossiliferous rocks.
3. Nevertheless, beneath the fossiliferous rocks lies a great thickness of unfossiliferous quartzites, sandstones, slates, gneiss, and granite, almost entirely azoic.

These researches consequently lend great support to the views of those geologists who find an explanation of the past history of the rocks in the present operations of nature—views which were held half a century ago by Sir Henry De la Beche, were advocated by Mr. Poulett Scrope, and were still more clearly developed by Sir Charles Lyell, who has so ably elaborated the doctrine of uniformitarianism originally enunciated by Hutton. Sir Charles has, indeed, survived the prejudices which at first opposed his views, and has lived to see most of the reputed heresies of his youth become established as the creed of every philosophical geologist.

[T. H. H.]

## GENERAL MONTHLY MEETING,

### Monday, February 1, 1875.

George Busk, Esq. F.R.S. Treasurer and Vice-President,
in the Chair.

Robert Dunlop Buchanan, Esq.
Mrs. Laurence Cave,
William Payne, Esq. F.C.S. F.R.G.S.

were *elected* Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, *viz.*:—

FROM

*The Secretary of State for India*—Rig Veda Sanhita. Edited by F. Max Müller. Vol. VI. 4to. 1874.
*The Lords of the Admiralty*—Reports from H.M.S. 'Challenger,' No. 2. fol. 1873.
*Accademia dei Lincei, Reale*—Atti. Anno XXVI. Sess. 3, 4. fol. 1874.
*Asiatic Society of Bengal*—Journal, 1874, Part 1; Part 2, No. 2. 8vo.
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*Astronomical Society, Royal*—Monthly Notices, Vol. XXXV. Nos. 1, 2. 8vo. 1874–5.
*British Architects, Royal Institute of*—Sessional Papers, 1874–5. Nos. 2, 3. 4, 5. 4to.
*Cape of Good Hope Observatory*—Cape Catalogue of 1159 Stars, 1860. 8vo. 1873.
*Chemical Society*—Journal for Dec. 1874, Jan. 1875. 8vo.
*Claudel, M. (the Author)*—Théorie des Parallèles selon les Géomètres Japonais. (K 100) 8vo. 1875.
*Comitato Geologica d'Italia*—Bolletini, 1874, Nos. 7, 8. 8vo.
*Dunant, M. Henri (the Author)*—Convention de Genève: un Souvenir de Solférino. 6e éd. 16to. Paris, 1873.
L. de Cazenove: La Guerre et l'Humanité au XIXe Siècle. (Convention, &c.) 8vo. Paris, 1874.
J. Kastner: Invention du Pyrophone: Expériences, &c. (L 16) 8vo. 1875.
*Editors*—American Journal of Science for Nov. 1874. 8vo.
Astronomical Register for Dec. 1874 and Jan. 1875. 8vo.
Athenæum for Dec. 1874 and Jan. 1875. 4to.
Chemical News for Dec. 1874 and Jan. 1875. 4to.
Engineer for Dec. 1874 and Jan. 1875. fol.
Journal for Applied Science for Dec. 1874 and Jan. 1875. fol.
Nature for Dec. 1874 and Jan. 1875. 4to.
Nautical Magazine for Dec. 1874 and Jan. 1875. 8vo.
Pharmaceutical Journal for Dec. 1874 and Jan. 1875. 8vo.
Practical Magazine, Jan. 1875. 4to.
Telegraphic Journal for Dec. 1874, and Jan. 1875. 8vo.

*Franklin Institute*—Journal, Nos. 586, 587. 8vo. 1874.

*Geographical Society, Royal*—Proceedings, Vol. XVIII. No. 3. 8vo. 1874.

*Geological Institute, Imperial, Vienna*—Verhandlungen, 1874, No. 12 et seq. 8vo.

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*Gladstone, Professor J. H. F.R.S. M.R.I.*—The Argonaut. Edited by G. Gladstone. Vol. I. 8vo. 1874.

*Greenwich Observatory*—Observations in 1872. 4to. 1874.

*Hayne, Charles Seale, Esq. M.R.I.*—Essayes of Natural Experiments made in the Academie del Cimento. Englished by R. Waller, F.R.S. 4to. 1684.

*Horticultural Society, Royal*—Journal, Part 15. 8vo. 1874.

*Irish Academy, Royal*—Proceedings, Nov. 1874. Vol. II. Ser. II. No. 1. 8vo. 1875.

*Lubbock, Sir John, Bart. M.P. F.R.S. M.R.I. (the Author)*—British Wild Flowers considered in relation to Insects. (Nature Series.) 16to. 1874.

*Manchester Philosophical Society*—Proceedings, Vol. XIII. No. 12. Vol. XIV. Nos. 5, 6. 8vo. 1874.

*Mechanical Engineers' Institution, Birmingham*—Proceedings, August, 1874. Part 1. 8vo.

*Northumberland, The Duke of, D.C.L. Pres. R.I.*—A Conference of Pleasure, composed for some Festive occasion about 1592, by Francis Bacon. Edited, from a manuscript belonging to the Duke of Northumberland, by James Spedding. 4to. 1870.

*Photographic Society*—Journal, Nos. 254, 525. 8vo. 1873.

*Physical Society of London*—Proceedings, Part 1. 8vo. 1874.

*Preussische Akademie der Wissenschaften*—Monatsberichte, Sept. Oct. 1874. 8vo.

*Prosser, R. B. Esq.*—Mr. Tatum's Lectures on Natural Philosophy, Electricity, Chemistry, and Physiology. (MSS.) 1810, 1811. 4to.

*Quaritch, Mr. B.*—A General Catalogue of Books. 8vo. 1874.

*Royal Society of London*—Proceedings, Nos. 156, 157. 8vo. 1874.

*Salomons, Sir David, Bart. M.R.I. (the Author)*—A New Method of Signalling on Railways. (K 103) 8vo. 1874.

*Smart, B. C. M.D. and H. T. Crofton, Esq. (the Authors)*—Dialect of the English Gipsies. 8vo. 1875.

*Symons, G. J. Esq. (the Author)*—Symons' Monthly Meteorological Magazine, Dec. 1874 and Jan. 1875. 8vo.

*Vereins zur Beförderung des Gewerbfleisses in Preussen*—Verhandlungen, Jan.–April. 1874. 4to.

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## WEEKLY EVENING MEETING,

Friday, February 5, 1875.

WARREN DE LA RUE, Esq. D.C.L. F.R.S. V.P. in the Chair.

### JAMES DEWAR, Esq. F.R.S.E.

### *On the Physiological Action of Light.*

THE late Sir HENRY HOLLAND, President of the Royal Institution, in his essay on the "Progress and Spirit of Physical Science" (1858) says, "The experiments of D'Arcy prove that the impression of light is often retained on the retina for fully two and a half minutes, the time in which a luminous particle or undulation passes through nearly thirty millions of miles of space! What is the condition of light—be it conceived as matter, or motion, or force—when thus arrested and enchained in a living organism?"

To this profound problem of the specific effect produced on the retina and optic nerve by the action of light, Mr. Dewar and Dr. John G. McKendrick have especially directed their attention. Numerous hypotheses have been made from time to time by physicists and physiologists; but up to the present date our knowledge of the subject is without any experimental foundation. For example, Newton, Melloni, and Seebeck stated that the action of light on the retina consisted of a communication of mere vibrations; Young conjectured that it was a minute intermittent motion of some portion of the optic nerve; Du Bois-Reymond attributed it to an electrical effect; Draper supposed that it depended on a heating effect on a choroid; and Mosier compared it to the action of light on a sensitive photographic plate.

It is evident that, in accordance with the principle of the transference of energy now universally accepted, the action of light on the retina must produce an equivalent result, which may be expressed, for example, as heat, chemical action, or electro-motive power. It is well known that the electro-motive force of a piece of muscle is diminished when it is caused to contract by its normal stimulus, the nervous energy conveyed along the nerve supplying it; and similarly a nerve suffers a diminution of its normal electro-motive force during action. In the same manner, the amount and variations of the electro-motive power of the optic nerve affected secondarily by the action of light on the retina, are physical expressions of certain changes produced in the latter; or, in other words, are functions of the external exciting energy, which in this case is light. Considerations such as these led them to form the opinion that the problem of what effect, if any, the action of light has on the electro-motive force of the retina and optic nerve, would require for its investigation very careful and refined experiment.

The inquiry divided itself into two parts—first, to ascertain the electro-motive force of the retina and nerve; and, second, to observe whether this was altered in amount by the action of light. The electro-motive force of any living tissue can be readily determined by the method of Du Bois-Reymond. This great physiologist found that every point of the external surface of the eyeball of a large tench was positive to the artificial transverse section of the optic nerve, but negative to the longitudinal section. This he accomplished by the use of his well-known non-polarizable electrodes, formed of troughs of zinc carefully amalgamated, containing a solution of neutral sulphate of zinc, and having cushions of Swedish filter paper on which to rest the preparation. (To protect the preparation from the irritant action of the sulphate of zinc, a thin film, or guard of sculptor's clay, moistened with a ·75 per cent. solution of common salt, and worked out to a point, was placed on each cushion.) These electrodes were connected with a galvanometer, and the preparation was placed so that the eyeball, carefully freed from muscle, rested on the one clay-guard, while the transverse section of the optic nerve was in contact with the other. By following Du Bois-Reymond's method the experimenters had no difficulty in obtaining a strong deflection from the eyes of various rabbits, a cat, a dog, a pigeon, a tortoise, numerous frogs, and a goldfish. The deflection was frequently so much as to drive the spot of light off the galvanometer scale.

With regard to the second question—namely, Whether, and to what extent, the electro-motive force would be affected by light? more difficulty was found. The method followed was to place the eyeball on the cushions in the manner above described, to note the deflection of the galvanometer needle, and then to observe whether or not any effect was produced on the impact of a beam of light during its continuance and on its removal. In a few of the earlier experiments Du Bois-Reymond's multiplying galvanometer was used, but finding the amount of deflection obtained was so small that the effect of light could not be readily observed, they latterly used Sir William Thomson's exceedingly sensitive reflecting galvanometer, kindly lent them by Professor Tait. They met also with secondary difficulties, such as the dying of the nerve, the impossibility of maintaining an absolutely constant zero, and an absolutely constant amount of polarity, the effects of heat, &c.; but these difficulties were overcome as far as possible by the most approved methods. The changes in polarity of the apparatus occurred slowly, and could not be mistaken for the changes produced by the action of light, which occurred suddenly, and lasted a short period of time. It is also important to state that the deflections observed do not at present profess to be absolute, but only relative values. About five hundred observations were made previous to April 1873, and every precaution was taken to obtain accurate results. The effects of heat were carefully avoided by covering over the troughs on which the eye under examination rested, with a spherical double-shell of glass, having at least an inch of water between the walls.

The results arrived at are as follows:

1. The action of light on the retina is to alter the amount of the electro-motive force to the extent of from 3 to 7 per cent. of the total amount of the natural current.

2. A flash of light, lasting the fraction of a second, produces a marked effect.

3. A lighted match, held at a distance of four or five feet, is sufficient to produce an effect.

4. The light of a small gas flame enclosed in a lantern, and caused to pass through a globular glass jar (12 inches in diameter), filled with a solution of ammoniacal sulphate of copper, or bichromate of potash, has also produced a change in the amount of the electro-motive power.

5. The action of light on the eye of the frog is as follows: When a diffused light is allowed to impinge on the eye of the frog, after it has arrived at a tolerably stable condition, the natural electro-motive power is in the first place increased, then diminished; during the continuance of light it is still slowly diminished to a point where it remains constant; and on the removal of light there is a sudden increase of the electro-motive power nearly up to its original position. The alterations above referred to are variables, depending on the quality and intensity of the light employed, the position of the eye-ball on the cushions, and modifications in the vitality of the tissues.

6. Similar experiments made with the eye of warm-blooded animals, placed on the cushions as rapidly as possible after the death of the animal, and under the same conditions, have never given an initial positive variation, as detailed in the case of the frog, but always a negative variation. The after inductive effect on the withdrawal of light occurs in the same way.

7. Many experiments were made as to effect of light from different portions of the spectrum. This was accomplished by causing different portions of the spectrum of the oxyhydrogen lime-light to impinge on the eye. All these observations tend to show that the greatest effect is produced by those parts of the spectrum that appear to consciousness to be the most luminous; namely, the yellow and the green.

8. Similarly, experiments made with light of varying intensity show that the physical effects observed vary in such a manner as to correspond closely with the values that would result if the well-known law of Fechner was approximately true.

9. The method followed in these inquiries is a new method in physiological research, and by the employment of proper appliances it may be greatly extended, not only with regard to vision, but also to the other senses.

After April 21, 1873, Mr. Dewar and Dr. McKendrick, the experimenters, endeavoured to obtain quantitative results, involving time as a variable element in the case of the action of light on the retina and optic nerve. They therefore found it necessary to construct a true graphical representation of the variations of the electro-motive force occasioned by the impact and cessation of light. It is clear that to register minute galvanometrical alterations, the only plan that could be employed would be to photograph on a sensitive surface, covering a cylinder rapidly revolving on a horizontal axis, the alteration of position of the spot of light reflected from the mirror, just as continuous magnetic observations are registered. As the apparatus required to execute these observations is very complicated, and would require much preliminary practice, they in the meantime adopted a simpler method of registration. This plan was to note the position of the galvanometer at equal intervals of time before, during, and after the impact of light on the eye. In these observations they used a seconds pendulum giving a loud beat. One observer read aloud the galvanometer; the other marked every interval of two and a half seconds, registered the numbers obtained, and regulated the supply of light. A little practice in the method above described enabled them to obtain very satisfactory results, agreeing very closely in different observations, and showing in a decided way the salient points of the variation curve.

These curves show that on the impact of light there is a sudden increase of the electro-motive force: during the continuance of light it falls to a minimum value, and on the withdrawal of light there is what they termed an *inductive effect*, that is to say, a sudden increase of the electro-motive force which enables the nerve to acquire its normal energy. The falling off of the electro-motive force by the continued action of light is the physical representative of what, in physiological language, is called fatigue; the inductive effect exhibiting the return of the structure to its normal state. Occasionally the impact of light is not followed by a rise in the electro-motive force, but by a diminution. This is probably to be explained by the fact, that the death of the retina and nerve is indicated by a gradual falling of the electro-motive force, and that this change frequently goes on so rapidly that the impact of light is unable to produce any rise. In these circumstances, the spot of light, which before the impact of light was slowly moving downwards, is on the impact steadied for a moment, and then pursues its downward course more rapidly.

By several distinct sets of observations:

1. It was proved that though there is no difficulty in obtaining a strong current from the skin of the frog, this current is not affected by light. This observation demonstrates that the pigment cells of the skin in the vicinity of the cornea have nothing to do with the results obtained.

2. The current obtained from a mass of the pigment cells of the choroid does not exhibit any sensitiveness to light.

3. The subcutaneous injection into the frog of woorara, santonin, belladonna, and Calabar bean, does not destroy the sensibility of the retina to light.

4. As to the action of the anterior portion of the eye. On carefully bisecting an eye of a frog, so as to remove completely the an-

terior portion, including cornea, aqueous humour, iris, ciliary muscle, and lens, and on bringing the retina into actual contact with one of the clay pads, we readily obtained a large deflection, which was as sensitive to light as when the whole eye was employed, thus eliminating any possibility of the contraction of the iris under the stimulus of light having to do with the results previously obtained.

5. On using the anterior portion of the eye, so that the cornea and posterior surface of the crystalline lens were the poles, a large deflection, which was, however, insensible to light, was obtained.

6. The sclerotic and nerve without the retina, in the same manner, gave a large natural electro-motive force, also not sensitive.

7. The distribution of the electro-motive force between the different portions of the eye and cross section of the nerve may be stated as follows: The most positive structure is the cornea, then the sclerotic, then the longitudinal surface of the nerve; the cornea is also positive to the posterior surface of the crystalline lens, and the retina itself seems to be positive to the transverse section of the nerve.

8. As to the effects produced by lights of different intensities. If a candle is placed at a distance of one foot from the eye, and then is removed ten feet, the amount of light received by the eye is exactly one hundredth part of what is got at a distance of one foot, whereas the electro-motive force, instead of being altered in the same proportion, is only reduced to one-third. Repeated experiments made with the eye in different positions have conclusively shown that a quantity of light one hundred times in excess of another quantity only modifies the electro-motive force to the extent of increasing it three times as much, certainly not more.

9. It was apparent that these experiments would ultimately bear upon the theory of sense-perception as connected with vision. It is now generally admitted that no image, as such, of an external object is conveyed to the sensorium, but that in reality the brain receives certain impressions of alterations taking place in the receiving organ. The natural query then arises, Are the physical effects we have described and measured really comparable in any way with our sensational differences in light perception, when we eliminate all mental processes of association, &c., and leave only perception of difference of intensity? In other words, are these changes the representative of what is conveyed to the sensorium? It would appear, at first sight, that this problem is altogether beyond experimental inquiry. There is, however, a way of arriving at very accurate measures of the variation of our sensational differences in the case of light, and this has been developed theoretically and experimentally by the justly renowned physiologist Fechner. Stating the law of Fechner * generally, we may say the difference of our sensations is proportional to the logarithm of the quotient of the respective luminous intensities. A recent series of experiments by Dalbœuf † has entirely confirmed the truth of this law. If, therefore, the observed differences in electro-

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* Fechner, 'Elemente des Psychophysik.' Helmholtz, 'Optique Physiologique.'

† Recent Memoir to Belgian Academy.

motive power, registered under conditions of varying luminous intensity, agree with this law of Fechner, regulating our sensational impressions, then there can be little doubt these variations are the cause of, and are comparable to, our perception of sensational differences. Now, it is stated above, that with a quantity of light one hundred times in excess of another quantity, the electro-motive force only becomes three times greater. According to Fechner's law, we may say the difference of our sensations, with that variation in the amount of luminous intensity, would be represented by 2, the logarithm of 100. The experimental results being as 3 to 1, the difference is also 2, thus agreeing very closely. It is to be remembered, however, that these results have been obtained by experiment on the eye of the frog, but similar changes have been observed in the eyes of mammals. In the latter, however, the amount of alteration is not so great, in all probability owing to the rapid death of the parts.

10. When one clay point is placed in contact with the cornea or nerve, and the other with the section of the optic lobe, a current is at once obtained which is sensitive to light. In this experiment the eye is left in the orbit, and the nerve is uninjured. Thus, the effect of light on the retina has been traced into the brain.

The continuance of these investigations led to the following results:

1. The light from a beam of uncondensed moonlight, though of weak intensity, and almost entirely free from heat rays, is still sufficient to alter the electro-motive power of the nerve and retina.

2. They examined the phenomenon in the eyes of the following animals: (1) The common newt—*Triton aquaticus;* (2) The goldfish—*Cyprinus auratus;* (3) The rockling—*Motella vulgaris;* (4) The stickleback—*Gasterosteus trachurus;* (5) The common edible crab—*Cancer pagurus;* (6) The swimming crab—*Portunus puber;* (7) The spider crab—*Hyas coarctatus;* (8) The hermit crab—*Pagurus Bernhardus;* and (9) The lobster—*Homarus vulgaris.*

The general results with the eyes of these various animals were similar to those previously described. The eye of the goldfish and rockling, both sluggish fishes, were found to resemble each other, inasmuch as the variations in the electro-motive force were slow, and in this respect they presented a marked contrast to those of the active and alert stickleback, the eye of which was very sensitive to light.

The experiments on the eyes of crustacea are of importance, because they show that the action of light on the compound eye is the same as on the simple eye, namely, that it alters the amount of the electro-motive force of the sensitive surface. The eye of the lobster was found to give a deflection of about 600 galvanometrical degrees, the scale being placed at a distance of about 26 inches. Light produced a variation in this deflection of about 60 degrees, that is, about 10 per cent., the largest amount of variation yet observed in any eye. It was also demonstrated that the effect of light, diminished in intensity by distance, was exactly what was observed in the case of the simple eye. For example, at the distance of one foot a variation to the extent of about 100 degrees was observed. At a distance of 10 feet, with $\frac{1}{100}$th part of the amount of light, the effect was not 1 degree but 20 degrees, or $\frac{1}{5}$th of the total amount observed at 1 foot.

3. The action of light on the electro-motive force of the living eye in cats and birds (pigeon and owl) has been observed. In the earlier experiments great difficulty was found in observing sensitiveness to light in the eyes of mammals and birds, when these were removed with the utmost dispatch from the orbit of the animal immediately after death. This was evidently owing to the fact that the sensibility of the nervous system in these animals disappears quickly after the withdrawal of healthy blood. It therefore became necessary to perform the experiment on the living animal. This was done by first putting the cat or bird under the influence of chloroform, then fixing it by a proper apparatus so that the head was perfectly immovable, and lastly removing the outer wall of the orbit with as little disturbance to the ciliary vessels as possible. The optic nerve was now cut, the transverse section directed upwards, and the clay points of the electrodes were now adjusted, one to the transverse section of the nerve, and the other to the cornea. With these arrangements a strong current extremely sensitive to light was at once formed.

4. The effect was traced into the optic lobes of a living pigeon under chloroform. The following were the results of this observation: *a*. When one pole was applied to the left optic lobe, and the other to the cornea of the right eye, a deflection was obtained which was sensitive to light: *b*. When the pole was removed from the right eye and applied to the cornea of the left, a smaller deflection was obtained, also sensitive to light; and *c*. When light was allowed to impinge on both eyes, while the one pole was in contact with either eye and the other with the left optic lobe, the result was nearly double that produced by the impact of light on one eye alone, either right or left. These effects may be explained by the decussation of the optic nerves in the optic commissure.

5. The eye of the snake* was examined, and in its action resembled that of the frog.

6. The law of the variation in the electro-motive force of the retina and optic nerve therefore holds good in the following groups of the animal kingdom, Mammalia, Aves, Reptilia, Amphibia, Pisces, and Crustacea.

7. Many experiments were made which prove that the psychophysical law of Fechner, previously alluded to, is not dependent only on perception in the brain, but in part on the structure of the eye itself. The effects which occur on, during, and after the action of light on the retina, also take place after the eye has been removed from all connection with the brain. Thus the law of Fechner is not, as has been hitherto supposed, a function of the brain alone, but is really a function of the terminal organ, the retina.

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* Kindly sent by Mr. Bartlett, of the Zoological Gardens, Regent's Park. Mr. Lloyd, Manager of the Crystal Palace Aquarium, kindly supplied three specimens of *Eledone* (a cuttle-fish, to represent *Mollusca*), but none arrived alive.

8. A new method was employed for registering galvanometrical variations, which may be of service in many physical and physiological researches. This consists in placing at the proper distance from the galvanometer, instead of the ordinary graduated scale, the surface of a cylinder covered with paper, and moving on a horizontal axis by clock-work. The spot of light reflected from the galvanometer mirror is rendered more precise by having the shade of the galvanometer lamp blackened over the entire surface, with the exception of a spot about three millimetres in breadth, in the centre of which a line or cross is made of soot. The image of this line or cross is of course reflected by the mirror upon the cylinder. When the cylinder is set in motion by the clock-work the spot of light may be accurately followed by the hand of the observer, after a little practice, with a fine brush moistened with ink. The cylinder employed performed a complete revolution in eighty seconds. This time was divided into four equal parts, each representing twenty seconds, by four lines drawn transversely at equal intervals across the paper on the cylinder. The first space, between lines one and two, represented twenty seconds, in which the eye was in the dark, and in which the electro-motive force is represented by a straight line; the second space, between lines two and three, represented twenty seconds, during which the effect of the impact of light took place, and in which the variation of the electro-motive force is indicated either by a curve to the right or to the left; the third space, between lines three and four, represented twenty seconds of continued action of light, during which the electro-motive force gradually rises; and lastly, the fourth space, between lines four and one (the point of starting), representing twenty seconds, during which the electro-motive forces at first rise on the withdrawal of light, and afterwards sink rapidly.

It has been experimentally proved—(1) that the impact of light on the eyes of members of the following groups of animals, viz. Mammalia, Aves, Reptilia, Amphibia, Pisces, and Crustacea, produces a variation amounting to from 3 to 10 per cent. of the normal electro-motive force existing between the corneal surface and the transverse section of the nerve; (2) that this electrical alteration may be traced into the brain; (3) that those rays that we regard as most luminous produce the largest variation; (4) that the alteration of the electrical effect with varying luminous intensity seems to follow very closely ratios given by the psychophysical law of Fechner; (5) that the electrical alteration is due to the action of light on the retinal structure itself, as it is independent of the anterior portion of the eye, eliminating, therefore, the natural supposition that the contraction of the iris might produce a similar result; (6) that it is possible, by experiment, to discover the physical expression of what is usually called in physiological language fatigue; and (7) that the method employed in this research may be applied to the investigation of the special organs of the other senses.

[J. D.]

## WEEKLY EVENING MEETING,

Friday, February 12, 1875.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

### W. R. Greg, Esq.

### *On Life at High-Pressure.*[*]

I. Beyond doubt, the most salient characteristic of life in this latter half of the nineteenth century is its speed—the rate at which we move and the high pressure at which we work. The question to be considered is, first, whether this is in itself a good; and, next, whether it is worth the price we pay for it. Unquestionably life seems certainly fuller and longer for this speed; is it really *richer* and more effective? We can do more; but is "doing" everything and "being" nothing?

Our pride in our rapidity of movement is natural, but not rational or healthy. [The speaker here quoted, in striking illustration of his meaning, Baron Hubner's account of his voyage across the Atlantic, where, in order to arrive forty-eight hours sooner, the steamers encounter dangers fitted to appal the stoutest heart.] The physical consequences of this needless haste and hurry are serious enough; but the moral consequences are probably graver still. Chronic disturbances of the nervous system and other evils, such as heart disease, ensue from rapid railway travelling; and a life of excitement, such as haste inevitably involves, can scarcely be deemed an adequate or a worthy life. An atmosphere of excitement, especially when we enter it young and continue in it habitually, is fatal to the higher and deeper life; it insidiously and gradually conduces to an unsound mental and moral condition. [Reference was here made to the scenes witnessed in France during the distressing years of 1870–71.]

II. But our life at high pressure is shown even more in our style of work than in our rate of movement. The world is growing more exacting in its demands from all labourers, except merely manual ones. [This was illustrated by reference to the increased strain put upon the powers of the professional classes—lawyers, physicians, and statesmen.] And it is not only health and strength that often give way. Men who give up their entire being to this labour often lose all capability of a better life, all relish for recreation or contemplation, all true appreciation of leisure, when it comes at last,—when the culminating point of success is reached. Thus it is that we sacrifice life to a living—the end to the means—

> "Et, propter vitam, vivendi perdere causas."

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[*] This discourse (in an enlarged form) was published in the 'Contemporary Review' for March, 1875.

People maintain that this excess of toil is unavoidable, and that you must keep the pace or fall behind. [The speaker was not convinced of this; and urged, that if it were so, men should not work over long; they should yield the place to younger and needier aspirants. His indictment against the spirit of the age is, that excess is enforced, and that moderation—which to the wiser Greeks seemed the essence of wisdom—is forbidden, or appears to be so.]

Another point to be noticed is, that while this high pressure assigns its prizes to men of exceptional physique, the average man succumbs. Even in intellectual careers, physical strength is nearly as essential as mental superiority, which often fails for want of it. Thus the slow-moving, the tardily-developing, now bid fair to be elbowed out of their careers, and the prospect before the dull and dunces is growing deplorable indeed.

III. It would seem that the future in England is not to be for the moderately wealthy any more than for the moderately industrious or moderately clever. The aggregate wealth of this country has enormously increased, and more vast fortunes have been heaped up in a shorter time than probably at any former epoch.* Increased riches have brought increased demand for most articles; and in those articles consumption has overtaken production; and many of these are articles of prime necessity,† with consequent higher prices and a rise in house-rent and servants' wages and maintenance; and among average middle-class families the cost of living is 25 per cent. higher than it was twenty-five years ago. With this the style of living also has advanced in an extraordinary ratio: persons who could live in comfort on 400*l.* or 600*l.* a year strive in vain to do so now; and there is a danger of the moderate, unstriving natures being crushed out between the upper and nether millstones of a prosperous, well-paid labouring class and the lavish noble or ignoble opulent.

As a remedy to this, the acknowledged chiefs of society should judiciously set an example by causing simplicity to become "good style," and luxury and ostentation to be voted vulgar. The seeds of this moral revulsion are already in existence; and there are thousands among our upper ranks to whom the indulgences and pleasures around them bring no true enjoyment, but rather the intense sadness of satiety.

In conclusion, the speaker expressed his strong aversion to asceticism—that is, to gratuitous self-denial or self-infliction—as an intellectual and moral mistake; and to the opposing demands of body and mind he applied the words, "Let us seek the conciliation of conflicting claims not by compromise, but by justice; by giving to everyone, not the half of what he asks, but the whole of what he ought to have."

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| In Millions. | 1858. | 1872. | Increase per cent. |
|---|---|---|---|
| * Property assessed to income tax .. .. | 327 | 482 | 47 |
| „ schedule D .. .. | 91 | 203 | 112 |
| | 1867. | 1872. | |
| † Live stock, i. e. cattle, sheep, and pigs, in the United Kingdom .. .. .. .. | 46,770,500 | 44,721,100 | |

## WEEKLY EVENING MEETING,

Friday, February 19, 1875.

The Duke of Northumberland, D.C.L. President, in the Chair.

Dr. E. Frankland, D.C.L. F.R.S. &c.

### *On River Pollution.*

In 1865 a Royal Commission was issued for the purpose of inquiring how far the present use of rivers in England, for the purpose of carrying off the drainage of towns and populous places, and the refuse arising from industrial processes and manufactures, can be prevented, without risk to the public health, or serious injury to such processes and manufactures, and how far such refuse and drainage can be got rid of or utilized, otherwise than by discharge into rivers, or rendered harmless before reaching them. Inquiry was also to be made into the water supply of Great Britain.

The Commissioners appointed were Mr. Robert Rawlinson, Mr. John Thornhill Harrison, and Professor Way. Their inquiry extended over three years, and they reported upon the state of the Thames, the Lea, the Aire, and the Calder, and their labours are recorded in three Blue Books with their appendices. This Commission was dissolved in 1868, and a new one appointed consisting of Major-General Sir William Denison, Mr. John Chalmers Morton, and the speaker. They were directed to complete the inquiries entrusted to the previous Commission and to extend them to Scotland. This second Commission finished its work in June last, or in little more than six years, and its labours are recorded in six Blue Books with their appendices.

Besides the inspection of river basins, towns, and manufactories, and the holding of courts of inquiry in the more important towns, these investigations involved the experimental examination of numerous processes for the cleansing of foul water, and the execution of thousands of analyses of foul and unpolluted water. For this purpose a thoroughly equipped chemical laboratory was provided by Government, furnished with every requisite for the investigation of water which modern science could suggest.

To properly consider the great mass of polluting matter which is being discharged into rivers and streams, it will be convenient to classify it into organic and mineral, according to its source and character. Under the head of organic matter we have, first, town drainage; second, drainage from the various forms of fibre manufacture, —and under this denomination come paper-making, calico industry, woollen industry, linen and jute industries, and the silk manufacture. Under the head of mineral matter we have, first, mine pollution, or liquids discharged from mines; and, second, drainage from chemical works.

As examples of rivers intensely polluted by each of the above forms of matter may be mentioned the Clyde, which flows through Glasgow, as strongly polluted by town drainage; Dighty Burn, near Dundee, by fibre manufacture; Red River, at Gwythian, Cornwall, by tin mines; and the Sankey Brook, which flows through St. Helens, is a fair example of pollution by chemical works.

Polluting matter of organic origin presents itself in water in two different forms, viz. in solution and suspension, whereas mineral polluting matter is nearly always present in a state of suspension, and on account of its greater specific gravity will, if allowed sufficient time, subside of its own accord. This is not the case with soluble or suspended organic matter, to get rid of which other means have to be resorted to.

Chemistry is not yet able to determine the actual weight of organic matter which may be present in solution in water. Only two of the principal elements, carbon and nitrogen, can as yet be determined; but the presence of these, even in comparatively small quantity, denotes formidable and offensive pollution.

The Thames may be taken as an instructive example of organic pollution. At its source it is a comparatively pure river. It receives polluting matters from paper factories, and from the drainage of 600,000 people, in its course before it reaches Hampton, and yet it still looks a comparatively clear and pure river. This is owing to the deceptive nature of the polluting matter, which is principally organic, and in solution, and is therefore scarcely discernible by the unaided senses. If, however, it be followed in its course down to London Bridge its pollution becomes apparently greatly augmented, but the organic matter in solution is scarcely perceptibly greater at London Bridge than at Hampton. In short, were the water filtered from the suspended mud stirred up by the steamers and currents, it would, chemically speaking, be nearly as pure at London Bridge as it is at Hampton.

Another instructive case is the Aire, which rises in Yorkshire, a very clear and beautiful river; but before it reaches Leeds it receives, besides the house drainage of more than a quarter of a million of people, the refuse from the following factories:

1341 cloth and woollen factories; 1 silk mill; 1 flax mill; 10 cotton factories; 7 paper mills; 26 tanneries; 13 chemical works; 8 grease works; 4 glue works; 35 dye works.

At one of the woollen factories alone the following materials are annually used:

Logwood and other dye woods, 320,000 lb.; chloride of lime, ammonia, and sulphuric acid, 15,000 lb.; Gallipoli oil, 40 to 50 tons; soap, 70,000 lb.; alkali, 40,000 lb.; and 14,000 tons of coal.

With the exception of the last item, nearly the whole of these materials are discharged into the river, and the ashes of the coal also find their way into the same convenient channel of transportation.

At Leeds this mass of pollution is reinforced by the drainage of 300,000 people, and by refuse materials from the following factories:

224 cloth and woollen factories; 62 dye works; 6 dye-wood mills; 25 flax mills; 7 soap works; 1 silk mill; 28 tanneries (which tan 2¾ million hides annually); 29 chemical works; 10 carpet factories; 3 glue factories.

The history of the river Calder, which joins the Aire lower down, is similar; but this river is less intensely polluted. After receiving the drainage of all the towns and factories on its banks, its water is pumped up for the supply of Wakefield.

The condition of the water may be judged of from the fact that a local manufacturer was able to write and dedicate a memorandum to the Local Board of Health with a pen dipped in the river water.

Now these two rivers—the Aire and Calder—the one running from Leeds, and the other from Wakefield, meet at Castleford, and there they fall over a high weir. At this spot the pollution of the water is so great as to blacken the very foam on its surface. Yet these streams must, at one time, have been celebrated for their cleanliness and purity; otherwise the well-known couplet would scarcely have been written:

> "Castleford lasses may well be fair,
> Wash'd in the Calder and bathed in the Aire."

The effect of bathing in these waters in their present condition would scarcely be so satisfactory.

The accounts of pollution just mentioned are those from town drainage and fibre factories, and these are essentially all organic. The pollution from mines and chemical works, with few exceptions, is mineral, and in suspension.

In the neighbourhood of mines the amount of mineral matter present in rivers is sometimes very great. There the ore and matrixes are crushed together into an impalpable powder, then washed with water to separate the heavier metallic matter from the rocky matter. The effluent water from the settling pits is very muddy, and often contains poisonous matters in suspension. This is especially the case in lead mines, where considerable quantities of galena and carbonate of lead, carried down the streams during floods, are washed on the adjacent land, where cattle are grazed. The consequences resulting from this display of ignorance and carelessness are, that the farmers whose lands are washed by these poison-charged waters suffer the loss of cattle and poultry, whilst the profits of the mines leak silently away. At a lead mine in Northumberland nearly 7 tons of lead ore, worth 12*l.* per ton, are thrown away in every 100 tons of waste material. At another mine in the same county 2 tons of lead ore, and more than 9 tons of zinc ore, are thrown away in every 100 tons of waste. In the Welsh mining districts there are also many examples of similar careless waste. These metalliferous "skimpings" and muds poison the rivers for many miles, carrying destruction to animal life. Mud containing as much as 5, 9, 13, and even 25 per cent. of lead ore is not unfrequently found in the neighbouring streams.

Such is the pitiable plight to which many of our formerly beautiful rivers have been reduced, and now the question confronts us,—What are the remedies for this grievous nuisance. Fortunately, science gives no uncertain reply to this question. Indeed, for one form—the casting of solid rubbish into streams—common sense, without the aid of science, supplies the answer. Such rubbish is laboriously and intentionally carried to the bank and shot into the stream, and its prohibition, under adequate penalties, needs only to be enforced.

The chief sources of river pollution are, first, town drainage and fibre manufacture; and, secondly, pollution by mining operations and mineral works. These are essentially different in their character, and require distinct remedial measures.

Before proceeding to investigate the various remedies which have been proposed for the first species of pollution, it is necessary to be quite sure that artificial remedies are necessary, and that Nature does not herself perform all that is really requisite for the purification of streams. It has been long a theory that polluted rivers cleanse themselves, and that if you pour into them foul organic matters, the latter are rapidly destroyed, and, after a flow of twelve miles or so, the rivers purge themselves completely from the stain, and regain their pristine purity,—an exceedingly comfortable doctrine, if true. This theory is in great repute amongst two classes of persons—first, those who are polluters of rivers; and, secondly, water companies who abstract their beverage from points below the outfalls of town sewage. Now considering that this is an exceedingly important question, the Commissioners submitted it to very close investigation. They experimented upon the Irwell, below Manchester; the Mersey, below Stockport; and the Darwen, below Blackburn. They took samples of the water at particular points of the rivers, and then at other points lower down, after a flow of a considerable number of miles, and before any other polluting matters had entered the stream. Thus, they took samples of the Irwell below Manchester, then eleven miles lower down, and found, that while there was a reduction in the organic matter in suspension, there was, practically, no diminution in the quantity in solution. The organic matter in solution was comparatively unaffected —certainly not destroyed, although the river falls over nine weirs, and is thoroughly aërated. The same results, substantially, were obtained in the Mersey, where there was a 13-mile flow available, and also in the Darwen, where there was also a 13-mile flow.

But it might be said that, in the case of these rivers, the pollution is so intense, that neither animal nor vegetable life can exist in them, and that it is the action of animal and vegetable organisms in water which destroys these pollutions. The Commissioners, therefore, resolved to repeat their experiments upon a river water in which pollution was much milder, and for this purpose selected the Thames.

They took first a sample of the Thames below Reading, after it is joined by the Kennet, and where it is polluted by the drainage of Reading.

Sufficient time was allowed for the river water to flow to a point four miles lower, and between these points no fresh polluting matter gained access to the stream. After this flow they found the quantity of organic matter in solution and suspension exactly the same as at the beginning, although the quantity of mineral matter in suspension was considerably reduced. Thus it will be seen that these experiments completely dispose of the theory of self-purification.

The methods which have been proposed for remedying organic pollution may be conveniently divided into, first, methods of precipitation; and, secondly, methods of oxidation.

Amongst the methods of purification those by precipitation have been most talked about, because they are the most easily applied on a small scale, and are, therefore, more easily made the subjects of experiment. One of these is precipitation by lime. Most drainage water contains bicarbonate of lime. This is decomposed by slaked lime or lime-water, and the resulting chalk precipitate lays hold of and carries down with it any particles of suspended matter that may be in the water. But it does more; it acts by surface attraction, and actually takes out of solution some of the organic matter which was dissolved in the water. Yet the purification by this means is very imperfect as regards the soluble organic matter.

There is, again, the A B C process, which consists in precipitation by means of a mixture of alum, clay, and an infinitesimal quantity of blood.

Then we have the sulphate of alumina method, or Bird's process; (lime and clay), or Scott's process; (lime and chloride of iron, and superphosphate of alumina and slaked lime). All these methods have failed to achieve the end in view.

They are useful in diminishing the suspended matter, but as regards purification from the worst form of pollution, viz. organic matter in solution, they are of little effect.

They have all had their origin in two erroneous ideas. First, that the foul matter in sewage can be removed by chemical means; and, secondly, that the matter actually thrown down or precipitated is valuable as manure. In every case in which it has been tried the precipitated matter is not worth the carriage for more than a mile or two, so that it is practically worthless. In fact, the manuring constituent of this foul drainage is left almost intact in solution. By General Scott's process, however, the precipitated matter is converted into a valuable cement.

The great lesson taught by all these comparatively abortive attempts is, that surface attraction is capable of effecting that which chemical affinity is incompetent to achieve. These are not cases of chemical affinity. There is no fixed relation between the matter precipitated and the precipitant. They are clear cases of surface attraction; but the vast quantities of purifying ingredients necessary to make them really efficient would involve so great an expense as to render them impracticable. Purifying material in each case is manufactured at great cost, and is laboriously carried to the sewage, and fished out again and dried at still greater trouble and expense. This, however, would become of secondary importance if the end desired were accomplished. But this is not the case;—the foul liquids are not cleansed. These purifying materials, chalk, alumina, and oxide of iron, and other porous substances exist, however, naturally in all porous soils, and sewage will run to them by its own gravity. These materials are capable of removing nearly the whole of the polluting matter brought into contact with them. We are thus led, by a process of scientific induction, to purification by irrigation. Foul drainage is, in irrigation, mixed with vast quantities of soil, which, by surface attraction, remove from it its polluting matter, both in solution and suspension. Its fertilizing matter is also removed to a great extent, and the roots of growing plants ramifying through the soil gather up and transform the polluting materials into healthy living tissues, thus preventing the pores of the soil from getting clogged and consequently useless for purposes of purification.

No one visiting Aldershot Camp can fail to be struck with the emerald tint of the irrigated land, contrasting as it does with the sterile soil around it. Upon the land used for the purpose of purification by irrigation every variety of crop can be grown, and at the Barking farm we can actually realize in the summer season the drainage of London transformed into strawberries and cream.

For the purpose of irrigation one acre of land is requisite for every hundred persons contributing to the drainage, and, when properly conducted, there is no nuisance. The only drawback to irrigation is the difficulty and sometimes the impossibility of obtaining sufficient land suitable for carrying on the process near large towns, and the absurdly high prices claimed for such land. This led to the experiment of making a given area of land do more work. After continuous filtration, however, the pores of the soil became clogged up, and the effluent water consequently not purified. The Commissioners therefore availed themselves of a well known property of porous matter, its attraction for gases, especially for atmospheric oxygen, and the great chemical affinity of this oxygen for organic matter. Spongy platinum possesses this property in a very high degree, and they were sanguine enough to hope that it might also be possessed by porous earth, to an extent sufficient to cause the slow but complete combustion of foul drainage matters.

Their expectation was not disappointed; experiments made upon sewage in their laboratory, and afterwards on the large scale at Merthyr Tydvil, showed that by bringing air and drainage water alternately in contact with the soil, rapid, continuous, and satisfactory purification was obtained. Indeed, the effluent water from the Merthyr drainage subjected to this process of purification is, chemically, purer than the water supplied by some of the London water companies.

This process is equally applicable to the discharges from fibre factories of various kinds. It has been carried out for three years at Merthyr Tydvil, where the drainage from each 3000 people has been cleansed upon a single acre of land. Crops may be also grown upon the land so used.

The Commissioners believe, as the result of their inquiries into these remedies for organic pollution, that the sewage purification of the future will be irrigation, but that for the present intermittent filtration is safer as regards expense. Neither process is any nuisance to the surrounding neighbourhood, but the carrying on of sewage farms by Corporations or Local Boards is a thing not likely to pay; amateur farming rarely does. By the process of intermittent filtration the original expense is much smaller as regards the quantity of land required. In the one case 30 acres are required for 3000 people, in the other only one acre for the same number.

For mining pollution the remedy is exceedingly simple, viz. subsidence in properly constructed tanks for six hours. The result, though not in every case quite satisfactory, is sufficiently so to prove that the amount of mining pollution in many of our rivers would, by its adoption, be substantially abolished. Even in the case of drainage from coal washing, the polluting matter after subsidence was reduced to a very small amount.

These plans would be sufficiently effective for the treatment of the chief kinds of town, manufacturing, and mining drainage. But it is quite necessary, in any legislative enactment to secure the better treatment of rivers, that there should be some definition of polluting matter. The Commissioners gave great attention to this point, and have proposed after much deliberation the following definitions of polluting liquids.

(*a*) Any liquid which has not been subjected to perfect rest in subsidence ponds of sufficient size, for a period of at least six hours, or which, having been so subjected to subsidence, contains in suspension more than one part by weight of dry organic matter in 100,000 parts by weight of the liquid; or which not having been so subjected to subsidence, contains in suspension more than three parts by weight of dry mineral matter, or one part by weight of dry organic matter in 100,000 parts by weight of the liquid.

(*b*) Any liquid containing, in solution, more than two parts by weight of organic carbon, or three parts by weight of organic nitrogen in 100,000 parts by weight.

(*c*) Any liquid which shall exhibit by daylight a distinct colour when a stratum of it one inch deep is placed in a white porcelain or earthenware vessel.

(*d*) Any liquid which contains, in solution, in 100,000 parts by weight, more than two parts by weight of any metal, except calcium, magnesium, potassium, and sodium.

(*e*) Any liquid which, in 100,000 parts by weight, contains, whether in solution or suspension, in chemical combination or otherwise, more than ·05 part by weight of metallic arsenic.

(*f*) Any liquid which, after acidification with sulphuric acid, contains, in 100,000 parts by weight, more than one part by weight of chlorine.

(*g*) Any liquid which contains, in 100,000 parts by weight, more than one part by weight of sulphur, in the condition either of sulphuretted hydrogen or of a soluble sulphuret.

(*h*) Any liquid possessing an acidity greater than that which is produced by adding two parts by weight of real muriatic acid to 1000 parts by weight of distilled water.

(*i*) Any liquid possessing an alkalinity greater than that produced by adding one part by weight of dry caustic soda to 1000 parts by weight of distilled water.

(*k*) Any liquid exhibiting a film of petroleum or hydrocarbon oil upon its surface, or containing, in suspension, in 100,000 parts, more than ·05 part of such oil.

In any enactment for the correction of river pollution, the above standards may be safely qualified by the following proviso: Provided always, that no effluent water shall be deemed polluting if it be not more contaminated with any of the above-named polluting ingredients than the stream or river into which it is discharged.

Of these standards the first two are by far the most important—the standards referring to suspended matter, and to the quantity of organic carbon and organic nitrogen which ought to be allowed to be transferred into streams. These supremely important standards, if enforced by proper and judicious enactments, giving sufficient time for manufacturers to carry out improvements necessary for the purification of foul drainage, would, we believe, abolish fully nine-tenths of all the river pollution by which our streams are now affected in Great Britain. But without the enforcing of the first two standards, this gigantic and growing evil, which is becoming greater and greater every year, will not be perceptibly diminished.

These investigations, extending over nearly ten years, show that the rivers of Great Britain are, in a very large number of cases, fouled and destroyed, not only for domestic, but also for manufacturing purposes, so that the extent of our manufactures is limited by the inadequate supply of pure water; and no people complain more loudly of this pollution than the manufacturers themselves, and none are more ready to adopt judicious measures for removing it. Various manufacturers have spoken of the value of the streams for them, if purified to the extent just specified, some of them estimating it at 1000*l*. per annum.

On the one hand, while the Commissioners have established this gross abuse of rivers where manufactures and mines are carried on, they have, on the other hand, very good reason to anticipate that a comparatively mild form of legislation, if firmly enforced, would have the effect of restoring many of the rivers to nearly their original purity. To make them pure for drinking purposes is, perhaps, impossible; but it may reasonably be hoped that they may become sufficiently so to delight the eye and to repress the pestiferous and sickening exhalations which at present affect the multitudes of our population compelled to pass their lives on the banks of such rivers.

[E. F.]

[illegible]

sees in the outlandish objects cast upon the strand by the Atlantic wave indications of the existence of a great and strange land beyond the western horizon. The tales current among each people, moreover, we are assured, may be ranked among its earliest records, and may be admitted to bear witness to the moral and intellectual character of its infancy and childhood; while from a comparison of the tales of various kindred races, we ought to be able to construct some kind of picture of the condition of the common stock from which those races have sprung. Thus the folk-tales of Europe are held to be part of the heirlooms of the Aryan family; their mutual resemblance, and their likeness to the similar tales of India and Persia, are supposed to point, as clearly as do linguistic affinities, to the far-off time when the ancestors of the Aryan races, which now occupy so vast a portion of the world, formed a united family party somewhere in Central Asia. There, at that remote period (we are told), the germs of what are now the folk-tales of Europe existed in the shape of myths, and each wave of emigrants from the cradle of the Aryan race carried them away together with their other heirlooms. From these germs have been independently developed the various stories now known as the folk-tales of the Aryan peoples of Europe—their common origin in prehistoric times fully accounting for the resemblance which runs through the numerous collections which have been made public. Just as in the various Aryan languages common roots have developed into words more or less different, but still bearing a strong family likeness, so, we are told, have common mythological germs independently developed in the minds of the various Aryan peoples into such stories as Cinderella, or The Sleeping Beauty, or Beauty and the Beast—stories, the different versions of which (to borrow an illustration from Professor Max Müller) differ indeed as sisters may, but are still marked by an unmistakable sisterly resemblance. For instance, it may be said, the germ of the Sleeping Beauty story probably existed long before the Aryan race split into Easterns and Westerns. From the earliest times the deathlike stagnation of the earth during winter had doubtless been symbolized under the form of a maiden's sleep; and the earth's renewed activity in spring, due to the vivifying influence of the sun, had been also symbolized under the form of that maiden's wakening, when roused by a lover's kiss. The elementary myths to which this symbolism gave rise were afterwards expanded by their different inheritors, at different times, into the stories, common to all the peoples of Europe, in which a sleeping beauty plays a prominent part. A similar process, it is argued, applied to other mythical germs of a similarly common nature, has produced the great mass of European folk-tales. These tales, it is maintained, have not been borrowed by one people from another. Each people, or at least each branch of the Indo-European race, has independently developed its popular tales. The differences which exist between the versions of the same tale in various lands are due to the diversities of thought and expression which characterize their kindred but long-divided inhabitants.

If all this be correct, it is evident that the folk-tales of Europe are the direct representatives of the wisdom of our ancestors as it existed in the infancy of that family of the human race to which we belong; that from their diligent study we may hope to glean such knowledge as may enable us to form some idea of the mental condition of those ancestors at a time when the distinctions were unknown which now separate the Hellenic, Italic, Keltic, Teutonic, Lettic, and Slavic families of speech or of mankind. These folk-tales must be regarded as the depositories of a body of invaluable evidence with respect to the ideas of our heathen forefathers about the material world and its spiritual counterpart, about the forms under which they embodied the forces of nature, the parables in which they conveyed their teaching as to man's relations with the unseen world, the condition of his spirit after its severance from his body.

The hypothesis which we have been considering is one which is at present widely, if not generally, received. But there is another method of accounting for the existence and the similarity of European folk-tales. According to it they do not, for the most part, belong by birthright to the European peoples, not having been by them developed from inherited germs, but having been borrowed from without, and merely adapted within. The same question was raised long ago with respect to the mediæval romances. At one time the Crusaders were credited with the introduction of the fictions on which those romances were founded; at another the Arabs who settled in Spain, or the Tartars who so long swayed a vast portion of eastern Europe. More recently the spread of the European folk-tales has been attributed to the same or similar agents.

Before proceeding to compare these hypotheses, it is necessary to explain with which species of folk-tale it is now proposed to deal. In every land there are found several tolerably well defined classes of Popular Tales—one consisting of what may be called anecdotes, brief, often credible, sometimes commonplace; another of what may be styled traditions, narratives told at greater length, but resembling the anecdotes in being generally of a possible nature, dealing to some extent with persons who have really existed, and events which have actually taken place. With neither of these classes are we at present concerned, nor with the beast-fables, nor with the religious legends. The stories to which it is proposed to restrict our attention this evening are the semi-dramatic romances of some length which occupy so large a space in all collections of folk-tales—stories containing a large infusion of the supernatural element, dealing with personages who are far above the laws of nature, and adventures which frankly set possibility at defiance. From among their number, moreover, only five specimens have been selected for detailed examination. But it may be safely asserted that almost all the European folk-tales of the class with which we are about to deal may be arranged in one of the two groups to which belong those selected stories, and that therefore the conclusions at which we may arrive in the case of the chosen few will hold equally good in that of the remaining many.

Before entering upon any detailed examination of the European folk-tales, some attention ought to be paid to the method employed in such investigations. There are a few words in one of the essays included in Professor Max Müller's 'Chips from a German Workshop' (vol. ii., p. 249) which ought to be carefully borne in mind by every story-investigator. It must be remembered, also, that the first duty of a commentator upon a folk-tale is to collect and compare numerous variants of it, in order to discover which among them is likely to be the oldest or the most elementary. Having done this, he may then venture to explore its hidden meaning. Neither task is an easy one. But the first process may be expedited by the application of the following tests to the confused mass of variants of any tale from among which it is required to extract the most primary form:

1. Is there any one among them which is thoroughly in keeping with the mythology, past or present, of the country in which it is found? If so, that is probably an original story, a native of the land in which it dwells. Let us call that country A.

2. Are the versions found in another country, which we will call B, at variance with what we know of the mythology of that country, but in keeping with the mythology of the other country, A? If so, they are probably natives of A, which have become naturalized in B.

3. Are the versions found in the four countries, B, C, D, and E, all at variance with what we know of the mythologies of those countries, but all more or less in keeping with the mythology of the country A? If so, they are probably all natives of A, naturalized in B, C, D, and E.

4. Do we find that the version discovered in A is rendered completely intelligible by our knowledge of A's mythology,—that A's mythology also serves to explain three-fourths of the most complete versions found in B, one-half of the similar version found in C, one-quarter of the similar version found in D, and scarcely any of the similar version found in E—none of the versions found in B, C, D, or E being rendered intelligible by the mythologies of those countries? If so, the chances are that the story has travelled from its original home, A, to its ultimate destination, E, *viâ* B, C, and D successively.

To use a metaphor. If we find a story running on all fours in A, halting on three legs in B, hobbling on two in C, hopping on one in D, and crawling legless in E—in that case we may fairly assume that it has performed a long journey from A to E, tarrying in B, C, and D successively, and becoming gradually dismembered on its way.

Now let us substitute for alphabetical signs geographical names. Let us suppose, for the sake of the argument, that the same story is found, under different forms, in India, Russia, Germany, France, and

England, and that the Indian story is thoroughly explained by, is in perfect harmony with, Indian mythology—while the European variants remain unintelligible when tested by what we know of the mythologies of heathen Europe, but are rendered at least partly intelligible by a knowledge of Indian mythology — then in that case we are entitled to hold that the story in question is a native of India, which has travelled thence to the European lands in question.

Finally, let us suppose (what in fact we are never likely to realize) that we find the story complete in all its members in India, but lopped of one limb in Russia, of two in Germany, of three in France, and utterly dismembered in England — then we may at least hazard a supposition that the story has travelled from India to England, *vià* Russia, Germany, and France successively.

In actual practice, it is true, the chain of evidence connecting a story with its home can seldom, if ever, be expected to prove so perfect as in this imaginary case. Different versions of a foreign tale may reach any given land by several distinct routes, especially if that land possess an accessible sea-coast. These versions may supplement or may confuse each other. Moreover, we can seldom feel sure that we possess the most complete form under which a folk-tale exists in a given land, and therefore we cannot place implicit reliance upon this method of route-tracing. From a great number of observations, however, we may often succeed in obtaining a sufficiently approximative result.

On single resemblances, unsupported by other evidence, too much stress must not be laid. Similar ideas may have sprung into life independently of each other in the minds of men widely separated by time or space, and may have been developed into similar but independent stories. Thus the similar caudal peculiarities of the bear and the hyæna may possibly have of themselves suggested the similar tales by which they are accounted for in Norway and in the heart of Africa. But it is not possible that out of similar ideas such complex narratives could ever have been independently evolved as Cinderella or Puss in Boots—narratives forming actual dramas, in the complete versions of which a considerable number of scenes lead in an almost invariable sequence, to all but identical results.

The conclusions to which I myself have been led by a comparison of several collections of folk-tales are the following:

1. The longer Popular Tales of the European peoples are, for the most part, not home products, but foreign importations. They have not been independently developed by those peoples from elementary myths common to all the branches of the Aryan family, but have been borrowed in an already developed form by those peoples, who have generally altered them so far as to adopt them to their own use.

2. They have been subjected to manifold influences, and in many cases have undergone repeated changes, so that it is often difficult, if not impossible, to trace the lines of their migrations. But, as a

general rule, those lines point steadily eastwards, and the country towards which most of them converge is India.

3. The light which these stories may possibly throw upon mythology falls almost exclusively upon Asiatic, generally upon Indian mythology. It is of but little use to seek in them information about the ancient religion of our ancestors, unless we wish to go back to a period anterior to the supposed settlement of those ancestors in Europe. It is worse than useless to draw from their study inferences relating to the religious beliefs of the Hellenic, or Italic, or Keltic, or Teutonic, or Letto-Slavic forefathers of the present inhabitants of Europe. If we insist upon doing so, we shall run the risk of attributing to the fierce sea-rover of Norway or the rude Slavonic herdsman ideas which are really due to the imagination of an Oriental novelist.

As regards the five stories selected for investigation, we may fairly assume that Cinderella, Beauty and the Beast, Blue Beard, and the Sleeping Beauty are mythological tales, or at least romances based upon mythological ideas. From Blue Beard the mythological element has been all but eliminated, and it has been so modified as to have become almost unrecognizable in the Western versions of Cinderella and Beauty and the Beast—the germ of the former story seeming to have been a myth describing the release of some supernatural being, generally of a demoniacal kind, from a forbidden chamber, and that of the two others a belief in the possibility of a divine personage electing or being compelled to assume the shape or husk of an inferior creature, human or brutish. The Sleeping Beauty may possibly be the expansion of a nature-myth, referring either to the nightly slumber of the earth till roused by the approach of the day, or its winter sleep protracted until the arrival of the spring. But Puss in Boots belongs to another order of tales, having originally been, beyond all doubt, a Buddhist parable—one belonging to the large cycle of apologues contrasting the gratitude of beasts with the ingratitude of man.

[W. R. S. R.]

## GENERAL MONTHLY MEETING,

Monday, March 1, 1875.

The **Duke of Northumberland**, D.C.L. President, in the Chair.

R. H. Alexander, Esq.
Blayney Balfour, Esq.
James Hill Bowen, Esq.
Mrs. Woodhouse Braine,
Thomas Browning, Esq.
Henry James Chaney, Esq.
Mrs. Thomas Jones,
Frederick Lehmann, Esq.
Mrs. G. J. Leon,
Mrs. George Henry Lewis,
Edward W. T. Hamilton, Esq.
Henry Rogers, Esq.
Mrs. Julia Scaramanga,
Sir John Scott, K.C.M.G.
George Fereday Smith, Esq. M.A. J.P. F.G.S.
Thomas Tomlinson, Esq. M.A.
James Vavasseur, Esq.
Hermann Weber, M.D.
John Westlake, Esq. Q.C.

were *elected* Members of the Royal Institution.

The special thanks of the Members were given to the Sir **Charles Wheatstone**, D.C.L. F.R.S. for his loan of an **Astronomical Clock** for one year.

The special thanks of the Members were given to the Rev. **Francis J. Holland** for his present of a copy of 'Fragmentary Papers on Science and other Subjects,' by his father, the late Sir **Henry Holland**, Bart. F.R.S. President of the Royal Institution.

### *Lecture Arrangements after Easter*, 1875.

**Professor P. M. Duncan**, F.R.S.—Second Series of Four Lectures on the Grander Phenomena of Physical Geography; on Tuesdays, April 6 to 27.

**John Hall Gladstone**, Esq. Ph.D. F.R.S. Fullerian Professor of Chemistry, R.I.—Five Lectures on Chemical Force; on Tuesdays, May 4 to June 1.

**Professor H. G. Seeley**, F.L. &. G.S.—Five Lectures on the Fossil Forms of Flying-Animals; on Thursdays, April 8 to May 6.

**James Dewar**, Esq. F.R.S.E.—Four Lectures on the Progress of Physico-Chemical Inquiry; on Thursdays, May 13 to June 3.

**George Smith**, Esq.—Three Lectures on Assyrian History; on Saturdays, April 10, 17, and 24.

THE REV. MARK PATTISON, B.D. Rector of Lincoln College, Oxford.—A Chapter of University History; on Saturday, May 8.

WALTER HERRIES POLLOCK, Esq.—Two Lectures on the Drama, on Saturdays, May 15 and 22.

PROFESSOR R. K. DOUGLAS.—Two Lectures on Chinese Language and Literature; on Saturdays, May 29 and June 5.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, *viz.*:—

FROM

*Actuaries, Institute of*—Journal, No. 97. 8vo. 1875.
*Astronomical Society, Royal*—Monthly Notices. Vol. XXXV. No. 3. 8vo. 1875.
*Bavarian Academy of Science*—Denkschrif tüber Liebig, von Dr. E. Erlenmeyer. 4to. 1874.
Annalen der Kön. Sternwarte bei München. Band XX. 8vo. 1874.
*British Museum Trustees*—Ancient Greek Inscriptions. Part I. fol.
*Chemical Society*—Journal for Feb. 1875. 8vo.
*Editors*—American Journal of Science for Dec. 1874. 8vo.
Athenæum for Feb. 1875. 4to.
Chemical News for Feb. 1875. 4to.
Engineer for Feb. 1875. fol.
Journal for Applied Science for Feb. 1875. fol.
Nature for Feb. 1875. 4to.
Nautical Magazine for Feb. 1875. 8vo.
Pharmaceutical Journal for Feb. 1875. 8vo.
Practical Magazine for Feb. 1875. 4to.
Telegraph Journal for Feb. 1875. 8vo.
*Franklin Institute*—Journal, No. 588. 8vo. 1875.
*Geographical Society, Royal*—Proceedings, Vol. XIX. No. 1. 8vo. 1875.
*Geological Society*—Quarterly Journal, No. 121. 8vo. 1875.
*Geological Society of Ireland*—Journal, Vol. XIV. Part 1. 8vo. 1874.
*Glasgow Philosophical Society*—Proceedings, Vol. IX. No. 2. 8vo. 1874.
*Harlem, Société des Sciences*—Verhandelingen. 3de Verz. Deel II. No. 3, 4. 4to. 1874.
Archives Néerlandaises, Tome VIII. Liv. 1, 2, 4, 5. Tome IX. Liv. 4, 5. 8vo. 1873–4.
*Holland, F. J. The Rev. (the Editor)*—Fragmentary Papers on Science and other Subjects, by the late Sir Henry Holland, Bart. 8vo. 1875.
*Lewis, T. R. Esq. (the Author)*—On Nematode Hæmatozoa. 8vo. 1874.
Microscopical and Physiological Researches on the Production of Cholera. 8vo. 1874.
*Linnean Society*—Journal, No. 78. 8vo. 1875.
*Manchester Geological Society*—Transactions, Vol. XIII. Parts 6, 7, 8. 8vo. 1874.
*Manchester Philosophical Society*—Proceedings, Vol. XIV. Nos. 7, 8. 8vo. 1875.
*Photographic Society*—Journal, No. 256. 8vo. 1873.
*Royal Society of London*—Proceedings, No. 158. 8vo. 1875.
Philosophical Transactions for 1874. Part II. 4to. 1874.
*Smithsonian Institution*—Smithsonian Contributions to Knowledge. Vol. XIX. 4to. 1874.
Smithsonian Miscellaneous Collections. Vols. XI. XII. 8vo. 1874.
*Statistical Society*—Journal, Vol. XXXVII. Part 4. 8vo. 1874.
Almanack for 1875.
*Symons, G. J. Esq. (the Author)*—Symons' Monthly Meteorological Magazine, Feb. 1875. 8vo.
*Upsala Royal Society of Sciences*—Nova Acta. Series III. Vol. IX. Fasc. 1. 4to. 1874.
Bulletin Météorologique Mensuel. Vol. V. Nos. 7–13. 4to. 1873.

## WEEKLY EVENING MEETING,

Friday, March 5, 1875.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The Lord Rayleigh, M.A. F.R.S. M.R.I.

### *On the Dissipation of Energy.*

The second law of thermodynamics, and the theory of dissipation founded upon it, has been for some years a favourite subject with mathematical physicists, but has not hitherto received full recognition from engineers and chemists, nor from the scientific public. And yet the question under what circumstances it is possible to obtain work from heat is of the first importance. Merely to know that when work is done by means of heat, a so-called equivalent of heat disappears is a very small part of what it concerns us to recognize.

A heat engine is an apparatus capable of doing work by means of heat supplied to it at a high temperature and abstracted at a lower, and thermodynamics shows that the fraction of the heat supplied capable of conversion into work depends on the limits of temperature between which the machine operates. A non-condensing steam engine is not, properly speaking, a heat engine at all, inasmuch as it requires to be supplied with water as well as heat, but it may be treated correctly as a heat engine giving up heat at 212° Fahr. This is the lower point of temperature. The higher is that at which the water boils in the boiler, perhaps 360° Fahr. The range of temperature available in a non-condensing steam engine is therefore small at best, and the importance of working at a high pressure is very apparent. In a condensing engine the heat may be delivered up at 80° Fahr.

It is a radical defect in the steam engine that the range of temperature between the furnace and the boiler is not utilized, and it is impossible to raise the temperature in the boiler to any great extent, in consequence of the tremendous pressure that would then be developed. There seems no escape from this difficulty but in the use of some other fluid, such as a hydrocarbon oil, of much higher boiling point. The engine would then consist of two parts—an oil engine taking in heat at a high temperature, and doing work by means of the fall of heat down to the point at which a steam engine becomes available, and secondly a steam engine receiving the heat given out by the oil engine and working down to the ordinary atmospheric temperature.

Heat engines may be worked backwards, so as by means of work to raise heat from a colder to a hotter body. This is the principle of the air or ether freezing machines now coming into extensive use. In this application a small quantity of work goes a long way, as the range of temperature through which the heat has to be raised is but small.

If the work required for the freezing machine is obtained from a steam engine, the final result of the operation is that a fall of heat in the prime mover is made to produce a rise of heat in the freezing machine, and the question arises whether this operation may be effected without the intervention of mechanical work. The problem here proposed is solved in Carré's freezing apparatus, described in most of the text-books on heat. There are two communicating vessels, A and B, which are used alternately as boiler and condenser. In the first part of the operation aqueous ammonia is heated in A, until the gas is driven off and condensed under considerable pressure in B, which is kept cool with water. Here we have a fall of heat, the absorption taking place at the high temperature and the emission at the lower. In the second part of the operation A is kept cool, and the water in it soon recovers its power of absorbing the ammonia gas, which rapidly distils over. The object to be cooled is placed in contact with B, and heat passes from the colder to the hotter body. Finally the apparatus is restored to its original condition, and therefore satisfies the definition of a heat engine. M. Carré has invented a continuously working machine on this principle, which is said to be very efficient.

Other freezing arrangements depending on solution or chemical action may be brought under the same principle, if the cycle of operations be made complete.

When heat passes from a hotter to a colder body without producing work, or some equivalent effect such as raising other heat from a colder to a hotter body, energy is said to be dissipated, and an opportunity of doing work has been lost never to return. If on the other hand the fall of heat is fully utilized, there is no dissipation, as the original condition of things might be restored at pleasure; but in practice the full amount of work can never be obtained, in consequence of friction and the other imperfections of our machines.

The prevention of unnecessary dissipation is the guide to economy of fuel in industrial operations. Of this a good example is afforded by the regenerating furnaces of Mr. Siemens, in which the burnt gases are passed through a passage stacked with fire-bricks, and are not allowed to escape until their temperature is reduced to a very moderate point. After a time the products of combustion are passed into another passage, and the unburnt gaseous fuel and air are introduced through that which has previously been heated. The efficiency of the arrangement depends in great degree on the fact that the cold fuel is brought first into contact with the colder parts of the flue, and does not take heat from the hotter parts until it has itself become hot.

In this way the fall of heat is never great, and there is comparatively little dissipation.

The principal difficulty in economy of fuel arises from the fact that the whole fall of heat from the temperature of the furnace is seldom available for one purpose. Thus in the iron smelting furnaces heat below the temperature of melting iron is absolutely useless. But when the spent gases are used for raising steam, the same heat is used over again at another part of its fall. There is no reason why this process should not be carried further. All the heat discharged from non-condensing steam engines, which is more than nine-tenths of the whole, might be used for warming or drying, or other operations in which only low temperature heat is necessary.

The chemical bearings of the theory of dissipation are very important, but have not hitherto received much attention. A chemical transformation is impossible, if its occurrence would involve the opposite of dissipation (for which there is no convenient word); but it is not true, on the other hand, that a transformation which would involve dissipation must necessarily take place. Otherwise the existence of explosives like gunpowder would be impossible. It is often stated that the development of heat is the criterion of the possibility of a proposed transformation, though exceptions to this rule are extremely well-known. It is sufficient to mention the solution of a salt in water. This operation involves dissipation, or it would not occur, and it is not difficult to see how work might have been obtained in the process. The water may be placed under a piston in a cylinder maintained at a rigorously constant temperature, and the piston slowly raised until all the water is evaporated, and its tension reduced to the point at which the salt would begin to absorb it at the temperature in question. After the salt and vapour are in contact the piston is made to descend until the solution is effected. In this process work is gained, since the pressure under the piston during the expansion is greater than at the corresponding stage of the contraction. If the salt is dissolved in the ordinary way energy is dissipated, an opportunity of doing work at the expense of low temperature heat has been missed and will not return.

The difficulty in applying thermodynamical principles to chemistry arises from the fact that chemical transformations cannot generally be supposed to take place in a reversible manner, even although unlimited time be allowed. Some progress has, however, recently been made, and the experiments of Debray on the influence of pressure on the evolution of carbonic anhydride from chalk throw considerable light on the matter. By properly accommodating the pressure and temperature, the constituents of chalk may be separated or recompounded without dissipation, or rather dissipation may theoretically be reduced without limit by making the operation slowly enough.

The possibility of chemical action must often depend on the density of the reacting substances. A mixture of oxygen and hydrogen in the proper proportions may be exploded by an electric spark at the atmospheric pressure, and energy will be dissipated. In this operation the spark itself need not be considered, as a given spark is capable of exploding any quantity of gas. Suppose, now, that previously to explosion the gas is expanded at constant temperature, and then after explosion brought back to the former volume. Since in the combination there is a condensation to two-thirds, the pressure required to compress the aqueous vapour is less than that exercised at the same volume by the uncombined gases, and accordingly work is gained on the whole. Hence the explosion in the expanded state involves less dissipation than in the condensed state, and the amount of the difference may be increased without limit by carrying the expansion far enough. It follows that beyond a certain point of rarity the explosion cannot be made, as it could not then involve any dissipation. But although the tendency to combine diminishes as the gas becomes rarer, the heat developed during the combination remains approximately constant.

It must be remembered that the heat of combination is generally developed at a high temperature, and that therefore work may be done during the cooling of the products of combustion. If, therefore, it is a necessity of the case that the act of combustion should take place at a high temperature, the possibility of explosion will cease at an earlier point of rarefaction than would otherwise have been the case.

It may probably be found that many mixtures which show no tendency to explode under ordinary conditions will become explosive when sufficiently condensed.

[R.]

## WEEKLY EVENING MEETING,

Friday, March 12, 1875.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

F. A. Abel, F.R.S. Treasurer C.S.

### *On Accidental Explosions.*

The winter of 1874–5 has been unusually prolific in so-called accidental explosions. On Christmas Eve a colliery explosion entailed the sacrifice of seventeen lives, and on one day last January the 'Times' published accounts of no less than six fatal explosions; two in collieries involving the loss of eleven lives; three boiler explosions, by one of which seven persons were killed and twenty injured; and one coal-gas explosion, by which two lives were lost. All these were of the class ordinarily termed accidental, but in every case the casualty was traced to a cause which could have been foreseen and guarded against.

The term "accident," applied in its strict sense to disasters caused by explosions, would imply that these were due to some circumstance, or combination of circumstances, entirely unforeseen, and that they were consequently unpreventable. An explosion which occurs during the preparation or investigation of a compound, the explosive nature of which is as yet unknown, may be purely accidental, but if, after the properties of the substance have been thoroughly ascertained and made known, an explosion occurs during its production, by some person who has not properly made himself acquainted with, or has neglected in some point or other, those conditions essential to its production with safety, the knowledge of which is within his reach, the term "accidental" can certainly not be properly applied to it, although in all probability it would be so designated popularly, and even by those entrusted on behalf of the public with the investigation of its origin and results. The explosion occasioned by a person employing a lighted candle to search for a gas-escape in a room where there is a powerful odour of gas, or a gunpowder explosion resulting from the dropping of a spark into an open powder-barrel from a candle which is held over it, should properly have as little claim to be called "accidental" as a boiler explosion occasioned by a person tying down the safety-valve. There may be criminal intent in the latter case which is entirely absent in the two former; but the probability of an explosion could have been equally foretold in all three instances, and though ignorance may perhaps be pleaded as an excuse in the case of the gas-explosion, and carelessness made to account for that of the powder, those pleas cannot be accepted as grounds for regarding the resulting disasters as legitimate accidents. Indeed it is often impossible to forbear stigmatizing as criminal the carelessness or ignorance which gives rise to explosions of the kind popularly accepted as accidental.

In the present discourse it is proposed to accept the definition "accidental" in the loose sense in which it is popularly applied to explosions, with the object of examining into the nature and causes of such explosions, and, if possible, of indicating directions in which there may be hope of successful efforts being made for reducing the frequency of their occurrence.

The phenomena attendant upon an explosion are generally due to the sudden or very rapid expansion of matter, accompanied in most instances by its change of state from solid or liquid to gas or vapour. The most simple classes of explosions are those caused by the sudden yielding to force, exerted from within, of receptacles in which a gas is imprisoned in a highly compressed condition, or a liquid has been raised to a temperature greatly exceeding that at which its molecules have a tendency to fly asunder or to assume the state of vapour or gas. The strength or elasticity of the envelope which confines them suddenly yielding to pressure, the liquid passes with great rapidity into vapour, violently displacing by this sudden expansion the surrounding air and any other obstacles opposed to the expanding molecules.

Similar explosive effects less simple in their origin are brought about by the sudden development of chemical activity in mixtures of gases or vapours, of solids and gases, or of solids only, or in chemical compounds of unstable character, the result in all such instances being the development of intense heat and the sudden or very rapid and great expansion of matter.

Examples of the most simple class of explosions are the sudden failure in strength at some particular point, or generally, of the material composing a vessel in which a gas has either been liquefied or highly compressed. Accidental explosions of this character take place chiefly, and happily not very frequently, in the laboratory or lecture room, yet instances occasionally occur of disastrous explosions resulting from such causes in manufacturing operations, or in the practical application of compressed air or other gases. The most recent illustration of a serious accidental explosion of this kind is that which occurred in the Arsenal at Woolwich, in January, 1874, with the air-chamber of a Whitehead, or Fish-Torpedo, when one man lost his life and several were seriously injured. In this instance some part of the soft steel diaphragm closing the chamber in which the motive power of this self-propellant torpedo (air) was imprisoned under a pressure of about 800 lb. on the square inch, suddenly yielded to the efforts of the gas to return to its normal condition.

These air-chambers were invariably tested up to a pressure very considerably exceeding that to which they would be subjected in actual service, before they were allowed to form part of the torpedo; yet in this instance, some comparatively weak point in the plate in question, though it had successfully resisted the force exerted by the very highly compressed air for a short period, was eventually tried beyond the limits of its strength, by the continued strain exerted by the air confined at a lower pressure; the powers of cohesion of its particles were suddenly overcome, when the confined air, released at one point, expanded with irresistible rapidity, tearing asunder the splendid metal which composed the air-chamber, and consequently inflicting fearful injuries on the poor fellows who were preparing to launch the torpedo on its trial trip. Most careful steps have been taken to avoid the recurrence of an accident of this nature, the possibility of which, with the precautions already adopted, was quite unforeseen.

Other explosions of this class, which are of more than weekly occurrence, and but too frequently result not merely in destruction of property, but in more or less serious loss of life, are due to the bursting of boilers at factories, mines, and collieries, to say nothing of those which occur in buildings, in connection with heating appliances and with kitchen ranges, and bath- or other heating-arrangements. The explosion of a boiler may arise either from an exceptionally rapid development of steam, the pressure speedily exceeding that which the boiler, or some portion of it, can withstand; or from an absence, or failure in the proper operation, of appliances for relieving the pressure in a boiler, by permitting the escape of steam, and giving warning when the pressure begins to exceed the limit of safety. The suddenly tumultuous evolution of steam may either occur from the boiler becoming overheated in parts, in consequence of the shortness of water, or from the sudden cracking or peeling of a non-conducting incrustation, or fur, on some part of the boiler, whereupon the water comes into contact with the overheated metal beneath it. But by far the chief causes of boiler explosions are defects in their construction, or repair, and the reduction in thickness of the metal in parts by corrosion, or oxidation, internally and externally, from long use, and neglect of proper measures for periodically cleaning the boilers.

The accidents due directly to the deposits formed from water in boilers have been very greatly diminished of late years by the application of preparations, called boiler-compositions, of which there are many varieties, their general action being to prevent, more or less effectually, the carbonate and sulphate of calcium and other impurities in water, which are separated by its ebullition and evaporation, from producing hard impenetrable crusts or coatings upon the inner surfaces of the boiler. The judicious employment of a good anti-fouling preparation, and the thorough periodical cleansing of the interior of boilers, go far to guard against that source of danger, though, in adopting measures to diminish the formation of incrustations, care must also be taken to avoid promoting internal corrosion of the boiler by the agents used.

The operations of the Manchester Steam Users Association for the prevention of steam-boiler explosions, founded, mainly through the instrumentality of Sir William Fairbairn, twenty years ago, and of which Sir Joseph Whitworth has also been a warm supporter from its commencement, appear to have gradually succeeded in very importantly reducing the annual number of boiler explosions, by introducing among its members a system of periodical independent inspection, the efficiency of which is clearly demonstrated by the circumstance that no case of explosion has yet occurred with boilers under the regular inspection of the Association, while between six hundred and eight hundred boilers are examined monthly. The periodical reports prepared by the talented chief engineer of the Association, Mr. Lavington Fletcher, are full of interesting and valuable information, and throw great light upon the chief causes of boiler explosions, and the means by which they may be guarded against. The Association will not allow that the term "accidental," or mysterious, is applicable to steam-boiler explosions. Mysterious they certainly are not, as they are generally quite traceable to causes which may be obviated, such as inferior material or defective construction, or local injuries, gradually developing and increasing, which would certainly be discovered before they attained dangerous dimensions, by a proper inspection; it may therefore not be too harsh to maintain that boiler explosions are not accidental, as their prevention lies in the power of the steam users, though it is true the same view must equally apply to other classes of so-called accidental explosions which are referred to in this discourse.

The following statements, furnished to the speaker by the Steam Users Association, afford some idea of the number and nature of boiler explosions which have occurred in the last few years. The number of explosions recorded annually by the Association, from 1865 to 1874 inclusive, ranges from forty-eight to seventy-two; the average present annual number is fifty. The largest number of deaths recorded is eighty-seven, and the smallest thirty-two, the present average number being seventy-five. About double that number of persons are seriously injured annually. With respect to the causes of boiler explosions, 40 per cent. were due (from January, 1861, to June, 1870) to malconstruction of the boilers; 29 per cent. to "defective condition" of the boilers; 15 per cent. to the failure of seams of rivets at the bottom of externally fired boilers; 10 per cent. to overheating from shortness of water; and less than 3 per cent. to accumulation of incrustations.*

An examination into the particular nature of the services performed by boilers which have exploded shows that a considerable number of explosions have occurred at iron works, and a very large proportion at collieries, where plain cylindrical externally-fired boilers are much used. Many of the explosions of these particular boilers arise from places which remain for a time concealed in the overlaps of the seams of rivets, defying detection, but gradually extending from one rivet-hole to another, till some sudden strain causes them to extend throughout the entire seam, the boiler splitting in two. The particular description of boiler which gave rise to the largest number of fatal accidents during the year taken as illustration (1873) was the single-flued or Cornish boiler; and it was stated by Mr. Fletcher that all these explosions must have been the result of glaring neglect, as there is no boiler safer to use when well made and properly cared for. The simple precaution of strengthening, or giving internal support to the sides of the furnace-tube of these boilers, the importance of which was demonstrated many years ago by Sir W. Fairbairn, appears to be still greatly neglected, the result being the frequent collapse of the tube through weakness. This illustration will suffice to indicate the nature of the "defects of construction" which are fruitful of the so-called accidents with boilers. An examination of the details given in Mr. Fletcher's reports as to the cause of explosions reveals a really appalling state of things under the head of "defective condition" of the boilers. In one case the plates are described as having been reduced by corrosion in places to the thickness of paper; in another, they were "in some places not more than from one-thirty-second to one-sixtieth of an inch thick;" in another, they were reduced at the primary rent "to the thickness of a knife-edge;" in another, the plates were in some places "eaten into holes which were roughly patched by means of bolted cement patches," while many of the rivet-heads were eaten off. These are merely examples of many similar descriptions. Very few explosions in 1873 appear to have been due to the neglect of the attendants, but by far the greater number to that of the boiler owners or the makers. The ignorance or criminal neglect, or worse, which appears occasionally to be displayed in the sale, purchase, and use of second-hand boilers is illustrated by the following two examples: A boiler which burst in December, 1873, on a rag-puller and waste dealer's premises, scalding three persons to death and injuring five others, was found to have given way on its first trial by its then owners, simply from old age and decay. At the earliest period at which its history could be traced it was purchased second-hand, and worked for five years; it was then left exposed to the weather for five years and afterwards sold to a broker for 5*l.*, who re-sold it as old iron for 8*l.* It was then sold for 18*l.* to an engineer, who sold it to its last owners with the assurance that it was safe at a pressure of 60 or 70 lb. to the square inch. The jury at the coroner's inquest returned a verdict of "accidental death," but "desired to express their disapprobation of the conduct of the engineer who sold the boiler as in good working order when the flue-tubes were in some places less than one-sixteenth of an inch thick." The other illustration is that of a large boiler which is graphically described in a professional journal of high standing as having been worked to within an inch of its life, being only removed from its seat because it would hardly hold water. On this boiler there was a patch, more than two feet long, covering a crack of the same length; the patch had only six bolts, three on each side, and was made tight with a piece of old carpet smeared with white lead. Had this boiler burst, the verdict would in all probability have been as in the other case "accidental death," though it was worked almost to the last to 35 lb. pressure.

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* These data are in part extracted from a Table submitted by Mr. Fletcher to the Select Committee on Boiler Explosions, in June, 1870, *vide* p. 118 of the Committee's Report.

The explosion early in January last of a boiler at North Shields, and of one at Sheffield, affords further illustration, if it were needed, of the fearful danger continually incurred, even by those well acquainted with the properties of iron and steel and the limits of their powers of resisting strain or pressure, in the employment of boilers long after they had ceased to be trustworthy. At Shields, at a large manufactory of chain cables and anchors, the boiler which exploded had been worked beyond a pressure of 35 lb., while it was unsound, over-patched, and quite unsafe at that pressure. Seven people were killed and about twenty others injured, and a vast amount of property was destroyed, the buildings within 500 yards appearing as though they had been bombarded. At Sheffield, where the explosion of a boiler not many days afterwards killed two persons, injuring several others, a leakage had been observed in the boiler some time before the explosion, but had not been repaired, and a plate fractured by the explosion had been greatly reduced in thickness by corrosion. This boiler appears to have been worked up to the day of the explosion at a pressure of from 40 to 50 lb., and the workmen employed are stated to have expressed fear at working in its vicinity.

The foregoing and other very numerous illustrations of the appalling display of ignorance, neglect, or recklessness in dealing with the application of steam-power, point strongly to the importance of legislation connected with this subject. A parliamentary committee of inquiry reported in July, 1870, that the introduction of compulsory independent inspection of boilers would interfere with the responsibility of the boiler owner; but it is somewhat difficult to realize where the responsibility lies, in the present condition of things. That a system of inspection may be made to work well has been demonstrated by the beneficial results of the now very widely applied labours of the Steam Users Association; and that Government realizes these benefits is demonstrated by the fact that the boilers at the manufacturing establishments of the War Department are subject to the inspection of the Association. There can be no reason why the responsibility of the proper condition of boilers, and steam apparatus generally, should not be thrown upon inspectors, just as the proper fencing of machinery in factories, and the proper condition of steam-boilers in a passenger steam-ship, are secured by a system of responsible official inspection.

The explosions which are often recorded as occurring in kitchen ranges and in boilers used in connection with the heating of buildings are not unfrequently attended by fatal results. Much of what has been said with regard to boiler explosions, generally, applies to accidents of this class. Explosions in such boilers have repeatedly arisen from corrosion of the metal, and consequent great reduction of its thickness in places, due to the accumulation of hard incrustations or furs which in time exclude the water from contact with the metal, causing the latter to scale from overheating. The incrustations will crack or split from time to time, in consequence of inequality of expansion or contraction between them and the metal, and if the latter be heated at the time, pressure of steam may be suddenly generated which the worn boiler will not withstand unless provided with an efficient safety-valve.

As the water in kitchen boilers is often used for culinary and drinking purposes, the means employed in boilers used for steam-purposes only, to prevent the formation of hard deposits, cannot be resorted to; therefore the only means of guarding against accidents to domestic boilers from these causes consists in frequent and thorough cleaning out, which is especially necessary where the water-supply is hard.

Explosions also occur with household boilers of the ordinary circulating class, unprovided with safety-valves, through the stop-taps of the pipes which connect them with an overhead cistern being left closed by accident or negligence, in which case steam pressure must speedily accumulate to a dangerous extent, all outlets being closed. Accidents with such boilers are particularly liable to occur during severe frosts in consequence of the circulating pipes becoming plugged up with ice, whereby the outlet for steam pressure is as completely cut off as if the stop-taps were closed. Several accidents due to these two causes, some of them attended by fatal results, were recorded last year. The obvious and simple method of guarding effectually against such explosions is to have the boiler fitted with a reliable safety-valve, of the most simple form; a valve of pendulous dead weight construction is specially recommended for this purpose by the Steam Users Association as being inexpensive, certain of action, and free from any liability to get out of order. So simple a precaution for avoiding the possibility of a frightful accident surely only requires public attention being directed to it to secure its general adoption.

Explosions resulting from the ignition of mixtures of inflammable gas and air constitute even a more formidable class than that just described; for the number of explosions in coal mines which occur in a year is very considerably greater than that of boiler explosions, while the loss of life occasioned by the former is very considerable, and is occasionally appalling in its magnitude. Thus, in 1871, there were fifty-two fatal explosions, 268 men being killed, and 234 non-fatal explosions, by which it is estimated that 368 men were injured; and, as examples of specially calamitous explosions, may be mentioned that at Risca Colliery in 1860, when 142 men were killed, and those at Oaks Colliery in 1866, and Ferndale Colliery in 1867, when 334 and 178 men were killed.

If marsh-gas, or light carburetted hydrogen, which exists imprisoned in coal-beds and escapes into the atmosphere in the pit-working, either gradually or sometimes under considerable pressure, becomes mixed with the air to such an extent that there are about eighteen volumes of the latter to one of the gas, the mixture burns with a pale blue flame, which will surround that of a candle contained in such an atmosphere; the appearance of such a "corpse light" round the flame of the pitman's candle or lamp-flame is a warning, too generally unheeded, of the presence of fire-damp in quantities likely to be dangerous; for if the proportion of marsh-gas increases much beyond that above specified, an explosive atmosphere will be formed, the violent character of which increases as the proportion of fire-damp approaches that of one volume to ten of air. Marsh-gas requires for its ignition to be brought into contact with a body raised to a white heat; fire-damp, or a mixture of marsh-gas and air, is therefore not inflamed by a spark or red-hot wire, but will explode if brought into contact with flame. The fact that this contact must be of some little duration to ensure the ignition of the fire-damp was applied by Stephenson in the construction of his safety-lamp; and a very philosophical application of the property possessed by good conducting bodies, such as copper or iron, of cooling down a flame below the igniting point of the gas, and thus extinguishing it, was made by Davy in the construction of his safety-lamp.

All the efforts of eminent scientific and practical men, for the better part of a century past, to diminish the number of coal-mine explosions, by improving the ventilation of the mines and providing the miner with comparatively safe means of illumination, appear to have had very little effect in reducing the number and disastrous nature of these accidents. Since the construction of safety miners' lamps by Davy, Stephenson, and Clanny, repeated and partially successful efforts have been made to reduce the loss of light consequent upon the necessary enclosure of the flame, and thus to lessen the temptation of the miner to employ a naked flame at his work in fiery mines. Yet investigations after mine explosions still frequently disclose instances of the employment of candles where they are undoubtedly dangerous; and the regulations which have been made law with the view of preventing accidents through the use of naked lights by miners, where there appears any likelihood of fire-damp escaping and lodging, are in many cases either habitually neglected or very carelessly carried out. One practice which appears to have become very general in mines where fire-damp is known to exist, that of sending *firemen* with safety-lamps to examine the mines, the men then proceeding to work with naked lights in all places marked as safe by those officials, is obviously a most dangerous one, the lives of many being made absolutely dependent upon the vigilance and trustworthiness of one or two; yet it appears to be one almost forced upon the managers of collieries by the men themselves, who often absolutely refuse to go to work with safety-lamps. Of the three colliery accidents which occurred between December 23rd and January 7th last, by which twenty-eight men lost their lives, two afford sad illustrations of the fact that the overlookers and the miners themselves are chiefly to blame for the frequency of these accidents, and that the practice of employing "firemen" just referred to is a highly perilous one.

In the case of the explosion at Bignall Hill Colliery, North Staffordshire, the Government Inspector gave evidence to the effect that the explosion occurred in what was admitted to be *the most fiery seam in the North Staffordshire coal-field*, and in which *no naked light ought ever to be used*. The overlooker stated that, having found gas in the heading on the morning of the explosion, he told the men not to go to work there with naked lights; that "he had authority to order lamps, but *he had not done so*, because he had not seen any necessity for it. The explosion might have been avoided if the man whose light was believed to have fired the gas, had done his duty and used a lamp." Another witness "had found gas on the morning of the explosion, but not sufficient to lead him to suspect danger. He found gas in the place after the fireman said he had cleared it away; ..... the workings always were considered safe, and if he asked a man to use a lamp he would often be laughed at."

The Government Inspector deposed that "all the circumstances went to show that the fireman did not examine the place before the men went to work, and that one of the men walked into it with a naked candle and ignited the gas." At the second explosion (at the Aldwarke Main Colliery near Rotherham) the fire-trier appears to have made his customary inspection and found all safe at the place where the explosion occurred, and had left his usual sign to indicate to the men that all was right. The opinion of the engineer was that a considerable fall of roof (of the working) had occurred after he had left, and that, before the liberated gas had time to be carried away by the ventilating current, the men entered and met the gas, which immediately fired. "Had there been a delay ..... or *had the men been provided with safety-lamps* the accident would in all probability have been averted." In the Report of the accident it is stated that the men in that district will rather leave a colliery than be subject to the strict enforcement of the rules of the Act passed to ensure their safety, and that some such calamity was needed to enforce proper regulations for the safe working of the mine. There can be no question that the comparatively dim light afforded even by the best constructed lamps in general use, is a cause of great temptation to the men to use uncovered lights; it is therefore much to be hoped that continued efforts may be made to apply the electric light to the illumination of mine workings. Some approach to success in this direction was already attained ten years ago, and one cannot but have great faith in the ultimate feasibility of some portable method of illumination by electric agency.

There are, however, causes other than the use of unprotected lights, which contribute to the production of coal-mine explosions. Efficient ventilation of workings, whether in use or not, whereby all dangerous accumulation of fire-damp is avoided and any sudden eruption of gas may be rapidly dealt with (the gas being largely diluted and swept away as speedily as possible) is indispensable to the safe working of the mine (without any reference to the health of the men) so long as there is any temptation for the use of naked lights. The original laying out of a working greatly affects the question of efficient ventilation, and explosions have been clearly traced to gas accumulations, which there was sufficient power of ventilation to reduce, if the nature of the working had admitted of its proper application. At the Bignall Hill Colliery where the fearful explosion occurred on Christmas Eve last, the ventilation had been reported imperfect about three weeks before the accident, and the Government Inspector stated that the deficient ventilation was due to the improper laying out of the work. In arranging for the efficient ventilation of a mine, ample provision for rapidly applying extra artificial ventilating power should be made; and, in connection with this, the interesting and useful series of observations should be borne in mind which have been made public in communications to the Royal Society and the Meteorological Society by Messrs. R. H. Scott and W. Galloway. Those gentlemen have clearly established a connection between the occurrence of meteorological changes and colliery explosions; they have shown that explosions frequently occur at the time of considerable barometric disturbances which must unquestionably affect the rate of diffusion of the fire-damp into air and the rapidity of air circulation in a mine. They also point out that when the external temperature rises very considerably, the natural ventilation, due to the higher temperature of the air in a mine, is liable to cease, by the external atmospheric temperature becoming equal to, or perhaps exceeding, that in the mine. They consider that of 233 explosions which occurred in 1872, 58 per cent. were due to disturbances of atmospheric pressure, and 16 per cent. to excessive heat of the weather, and they have similarly traced the proportion in which the same meteorological changes have contributed to bring about explosions in other years.*

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* Reference must be made here to an ingenious application, made by Mr. Ansell, of the laws of diffusion and osmose for detecting the presence of fire-damp in coal-pits. Two or three forms have been given by Mr. Ansell to the detector apparatus; perhaps the most simple consists of a ball of thin india-rubber, filled with air and fixed upon a stand under a lever which slightly presses the upper surface of the ball. If from any cause the lever is raised it liberates a spring which sets a bell in vibration, or brings an electro-magnetic alarum arrangement into operation. If this apparatus is placed where fire-damp exists to an important extent, the light carburetted hydrogen passes through the pores of the india-rubber ball, and as the air in the latter passes outwards at a much lower rate, the ball

Since the employment of gunpowder as a means of rapidly removing coal, or overlying shale, has come into considerable use, there can be no question that an additional and a very serious source of danger has been imported into the working of collieries. That the explosion of a charge of powder in a blast-hole, or the "firing of a shot," has by no means unfrequently resulted in the production of a fire-damp explosion, has been clearly established by careful inquiry. This has been ascribed to two causes, one of them the direct ignition of the explosive gas-mixture by the flame from the shot, the other the dislodgment of fire-damp from cavities or disused workings by the concussion produced, and its ignition by some naked flame or defective lamp in the neighbourhood. If a shot takes effect properly (i.e. if the force is fully expended in breaking the coal or rock at the seat of the charge), there is seldom flame produced; but if the tamping which confines the charge in the blast-hole is simply blown out of the latter like a shot from a gun (which not unfrequently occurs when the rock is very hard or the tamping is not sufficiently firm, or when the charge of powder is excessive), the powder-gas issuing from the blast-hole will produce a flash of fire as obtained with a gun; and if the fire-damp were in the immediate neighbourhood, it would no doubt be ignited thereby. But this combination of conditions is not likely frequently to occur; the second cause above given is therefore more likely to be fruitful of accidents: but the existence of a third cause, to which the majority of explosions connected with blasting in collieries is most probably ascribable, has been very clearly established by the careful inquiries, sound reasoning, and ingenious experiments of Mr. W. Galloway, Inspector of Mines. The fact has long been known that if an explosive gas-mixture be driven, in a current of sufficient force, through the gauze of a safety-lamp at right angles to the lamp, flame may be forced through the meshes, and may thus ignite the explosive mixture outside the lamp; but Mr. Galloway conceived, and has clearly established by experiments in the laboratory and in coal-pits, that the sound-wave established by the firing of a shot (especially by the sharp explosion produced when the tamping is shot out of a hole) will by transmission, even to very considerable distances, have the effect of forcing flame through the meshes of the gauze of a safety-lamp, and will thus lead to the ignition of an explosive gas-mixture surrounding the latter. Safety-lamps may frequently be accidentally surrounded by (or purposely introduced, for purposes of inspection, into) an explosive atmosphere at the time that shots are fired in neighbouring workings; and the circumstantial evidence collected after the occurrence of some explosions has undoubtedly

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soon distends, exerting sufficient pressure on the lever to bring into action the signalling arrangement, which, if electric, may be placed at any convenient distance from the fire-damp detector, being connected with the lever by means of conducting wires. It would appear as though a detector of this nature could be made of important service in fiery mines; but its employment does not seem as yet to have passed beyond the experimental stage.

indicated that this is the most probable explanation of the cause of the accident. It is probably not to be expected that even in fiery coal-pits miners will forego the advantages derivable from the employment of gunpowder, and it appears next to impossible to enforce prohibitory regulations with respect to such pits; but it may perhaps at any rate be hoped that the miner may be trained to a knowledge of the danger he incurs by the incautious use of gunpowder, although the persistent recklessness with which he sacrifices safety to comfort, in despising the use of the safety-lamp, forbids sanguine expectations in this direction.

Reference has not been made to another very possible source of accidents, due to the employment of gunpowder for blasting purposes, namely, carelessness in the keeping and handling of the explosive agent by the men. Personal observation by the speaker of the reckless manner in which powder is frequently dealt with in mines, leads him to believe that this contributes its quota as a cause of colliery explosions.

The accidents in collieries have their parallel in domestic life, in coal-gas explosions, which, though at first sight of comparatively small importance, if judged by the loss of life and property which they occasion, yet merit serious consideration on account of the great frequency of their occurrence, and the demonstration which they almost always afford of ignorance or culpable carelessness. Whether occasioned by defective gas-fittings or by want of care in dealing with the gas-supply, the escape of gas in a confined space, in any but very small quantities, must speedily result in the production of an explosive gas-mixture, by the rapidity with which the gas will diffuse itself through the atmospheric air surrounding the point of escape. The application of flame to such a mixture must inevitably result in an explosion, the destructive effect of which is regulated by the quantity of gas escaping, and the time which it has had to mix with the air.

The circumstance that the admixture of even minute quantities of coal-gas with air can be at once detected by the unmistakable odour of the gas, should serve as a safeguard against accidents; unfortunately, however, thoughtlessness or want of knowledge frequently causes this very fact to lead to the opposite result. Escapes of gas in comparatively small quantities often occur at the point of union (generally by a ball-and-socket joint) of a hanging burner or chandelier with the gas-pipe, or at the telescope-joint of such gas-fittings; the column of water required in the joints to confine the gas becoming very gradually reduced by evaporation. In such instances an explosive mixture will accumulate in the upper part of an apartment of which windows and doors are closed, while the air in the lower part will continue for a long time free from any dangerous admixture of gas; and instances are continually recorded in the public prints of the deliberate ignition of such explosive mixtures, by persons who, observing the smell of coal-gas upon entering the room, proceed forthwith to search for the point of escape by means of a flame. It need scarcely be stated that such a test is a perfectly safe one in itself, and that if the acceptance of the warning given by the odour of gas in the lower part of the room were promptly followed by the simple precautionary operation of leaving open for some time all windows and doors, so as to afford ready ingress of fresh air, and thus speedily expel, or very largely dilute, the gas-mixture, the leakage could be looked for with no risk of accident.

Gas explosions, generally of a serious nature, do occasionally occur through no fault of those who are the direct agents in bringing them about, as by a person entering with a light a closed apartment in which there has been a very considerable escape of gas for some time, or a building into which gas has been entering from a leakage in the supply-pipe or the main. A very sad accident of the latter kind occurred in January last at Durham, resulting in the death of two persons. The occupants of a house had noticed on a Saturday afternoon a smell of gas, which was traced to a leakage in the main-pipe by the gas company's servants, who stated that it could not be repaired until the following Monday. The man, being satisfied that the gas-escape did not occur in his house, lighted a candle some time afterwards, with the result that a terrific explosion nearly wrecked the house, which had become to a very considerable extent filled with explosive gas-mixture. In another recent instance, the accidental ignition of an explosive coal-gas mixture issuing from a drain-pipe communicating to the sewer in which gas had leaked from the main, caused the demolition of one house and the partial demolition of another, the inmates, in both instances, escaping fortunately with bad injuries. The first of these accidents, at any rate, would not have occurred if prompt means had been taken for stopping the leakage when its source was discovered; and it may be confidently affirmed that a very large majority of the accidents resulting from coal-gas explosions might be avoided by the exercise of ordinary care and intelligence.

The employment of illuminating agents closely allied to coal-gas, namely, liquid carbo-hydrogen compounds obtained by the distillation of coal or shale, or derived as natural products from coal-bearing strata, gradually extended during the earlier part of the last quarter of a century until they became formidable rivals of mineral and vegetable oils and even of gas itself. Among the very earliest of these so-called mineral oils was paraffin oil, first obtained from a particular description of schist or coal, by Mr. Young; but some time before then, the most volatile products obtained by the distillation of coal-tar were employed in lamps without wicks, of such construction that the volatile liquid, flowing by gravitation from the reservoir to the source of heat, was converted into vapour as it reached the point where the jets of flame were produced. The great facility with which the more volatile portions of rock-oil or petroleum, and of the products of distillation from coal and schist, were applicable as brilliant illuminating agents in lamps of simple, cheap, and convenient construction, combined with the low price at which they could be retailed, consequent upon the great development of the petroleum industry in America, soon led to their extensive employment by the general public, and the importation of these volatile oils or spirits attained in a short time a scale of considerable magnitude.

The several varieties of so-called petroleum spirit which are known as naphtha, benzine, benzoline, gasoline, japanner's spirit, &c., yield vapour more or less freely on exposure to air at ordinary atmospheric temperatures, and even in some cases below 50° Fahr. Their low specific gravity and volatility permit of their employment in very simple and inexpensive lamps—the commonest form now extensively used by the poorer classes all over the country being the "sponge lamp" in which a small strip of sponge or coarsely plaited wick serves the purpose of drawing the spirit up from the reservoir to the orifice, where it is vaporized and burned. Although much the largest proportion of the petroleum spirit employed is probably used in lamps of some form or other, there are additional important uses to which it is applied in large quantities, e. g. as a detergent, by dyers, &c.; as a substitute for turpentine in paints and varnishes; as a means of imparting luminosity to non-luminous inflammable gas, or of rendering atmospheric air itself the vehicle for illumination by saturating it with benzoline vapour; as a solvent and purifying agent in connection with the manufacture of artificial alizarine, &c.

The statistics of the imports into London and Liverpool during the last three years show a very considerable increase in the consumption of petroleum spirit, as will be seen from the following numbers:

| | 1872. | 1873. | 1874. | |
|---|---|---|---|---|
| Barrels imported .. | 33,693 | 45,889 | 58,687 | London. |
| | 26,871 | 27,394 | 47,200 | Liverpool. |

The imports of the less volatile and properly refined petroleum products, known generally as petroleum oil (also called kerosine, crystal oil, rock oil, astral oil, and, incorrectly, paraffin oil), which do not yield vapour below a temperature of 100° Fahr., have fluctuated considerably during the last six years, but have increased lately to about five-fold into London, and four-fold into Liverpool, of what they were in 1872, as shown by the following numbers:

| | 1872. | 1873. | 1874. | |
|---|---|---|---|---|
| Barrels imported .. | 48,295 | 206,573 | 245,291 | London. |
| | 46,679 | 104,155 | 159,700 | Liverpool. |

The so-called paraffin- or petroleum-lamp *explosions*, of which in the earlier days of the employment of these illuminating agents there were so many recorded in the newspapers, and of which one still hears occasionally, were, with very few exceptions, not correctly designated as explosions, and when they were so, were not caused by the employment of the volatile oils or petroleum spirit. As these vaporize very freely at the slightly elevated temperature which a reservoir of a lamp soon attains, air is either entirely expelled from the latter by the vapour, or so diluted by it, that the mixture is not explosive. If therefore flame can have access to vapour escaping from any opening in the reservoir near the wick, in a badly-constructed lamp, it will merely burn as it escapes. If a lamp charged with petroleum spirit be carried incautiously, or accidentally jerked so that the liquid is suddenly brought into contact with the warmer portion of the lamp, near the flame, a very rapid volatilization may thereby be caused, resulting in a considerable outburst of flame. In either case, a nervous person, holding the lamp at the time, may drop it in alarm, or the reservoir, if of glass, may be broken by the sudden burst of vapour and the resulting large flame; in such instances, the spirit will be scattered and at once inflamed, inflicting injuries more or less severe on the instigator of the accident, or setting fire to the premises.* A so-called petroleum-lamp explosion which occurred, quite recently, at Gloucester, resulting in the death, from burns, of a woman, was evidently brought about by the cause last described.

If a petroleum oil which has been imperfectly refined, and which therefore contains some proportion of the very volatile products, or spirit, be employed in a lamp, a slight explosion may be caused by its yielding up a small amount of vapour at the temperature to which the reservoir becomes heated, and thus producing a feebly explosive mixture with the air in the latter, which may become ignited by the flame of the lamp. An explosion thus produced is not at all of violent character, being generally merely a feeble puff; it may, however, cause the cracking of the reservoir, and the consequent spilling and inflaming of the oil, and may at any rate lead to accident as already described, by the alarm which it occasions to nervous or ignorant persons.

There is no doubt whatever that a very considerable proportion of the accidents which occur to persons using petroleum-lamps are really traceable to the erroneous belief, which is still so very prevalent, in the *explosive* character of these liquids. The fact that they and their vapours are simply inflammable, and that the latter require to be mixed with a large volume of air before their ignition can be accompanied by explosive effects, is so slowly realized, that in public prints petroleum is still often spoken of as an explosive substance, although these very prints have recorded such events as the *burning*, in 1866, of warehouses at Bow containing 1000 barrels of petroleum, besides four barges laden with petroleum lying alongside the wharf, without any explosion; and the ignition at New Jersey (in August, 1874) of a petroleum tank in the Erie Railway Company's oil-yard, and the consequent *burning* of 70,000 barrels of petroleum. The popular belief in the explosiveness of these simply inflammable liquids contrasts strangely with the fact that many explosions have been brought about by the careless employment of candles or other naked flames in premises where the volatile varieties have been stored, or where the operation of transferring the liquid from one vessel to another for purposes of sale is carried on, the result being the ignition of the explosive mixture *produced* by the volatilization of the spirit and its diffusion through the air. This fact does indeed tend to discourage the hope that the proportion of accidental explosions of *gunpowder*, which are apparently due to ignorance, may become very greatly diminished by keeping its explosive properties before the minds of those using it.

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* The Reports by Captain Shaw to the Metropolitan Board of Works of the causes of fires in London, show a generally steady increase in the number of fires caused by lamps: thus there were 20 in 1866, 30 in 1869, 49 in 1871, 56 in 1872, 47 in 1873, and 67 in 1874. These returns include fires from lamps of all kinds (many not defined); but it may be reasonably assumed that three-fourths of the totals noted were caused by petroleum-lamps. From one to seven fires annually are reported as caused by upsetting, "drawing off," or explosion of naphtha, paraffin, crystal oil, or petroleum. The proportion which fires from the above causes bear to fires caused by candles is small, but has been on the increase in the last few years. From 1866 to 1870 inclusive, the fires caused by candles were from seven to ten times more numerous than those caused by lamps; but in 1872 they were only about five times; in 1872 and 1873, four times; and in 1874 only little over three times more numerous.

Although those petroleum accidents which are the results of carelessness or ignorance are very difficult to attack, the community has unquestionably benefited very considerably in the matter of optional comparative security by the legislation of the last few years, in connection with the storage and sale of petroleum, imperfect though it still is. The prohibition of the sale of petroleum spirit of any kind, unless distinctly labelled so that the public may be alive to its specially dangerous nature as a very volatile liquid, and the limitation of its storage and sale, by properly licensed persons, to localities specially selected and inspected by responsible officials, have most certainly contributed greatly to the protection of the public against accidents entailed by the use of these materials. A still more important benefit which has resulted from legislation on this subject, aided by the zealous action of the Petroleum Association and of municipal authorities, is the almost complete exclusion from the market of such petroleum *oil* as yields inflammable vapour at temperatures considerably above the maximum atmospheric temperatures of Great Britain. The danger arising more especially from the transport and storage of oils bearing names which apply to the properly refined and therefore safe petroleum- or coal-oils which do not demand special precautions for their safe storage and use, and are consequently not subject to any restrictive or precautionary regulations, renders the application of the existing legal regulations to the inspection of petroleum *oils* imported into England of special importance. Such inspection, if efficiently conducted, must afford important protection to consumers and communities, by maintaining strictly the line of demarcation which it has been the object of legislation to establish between very volatile and therefore highly inflammable petroleum-, or coal-products, and those liquid hydro-carbons of the same origin which are comparatively very safe illuminating materials, because they do not evolve vapour at any temperature below about 100° Fahr. A thoroughly trustworthy and sufficiently simple method of testing the oils is essential to the attainment of this result; and after much consideration and practical experience had been brought to bear on this subject, the so-called *flashing test*, described in the Act of Parliament, was adopted as the fairest and most reliable one which could be prescribed for general application; but there is no doubt that, when applied to oils which nearly approach in their flashing point the limit prescribed by the Act, the operation of the test is not sufficiently sharp, nor the prescribed method of conducting it sufficiently rigid, to preclude the possibility of its furnishing conflicting results in different hands, and thus leading to decisions, the justness of which may be legitimately open to dispute. It is therefore undoubtedly desirable, in the framing of any future Act, that this test should be carefully reconsidered, as well as the question whether some narrow limit below 100° Fahr. may not reasonably, and without incurring any increased risk, be fixed within which the flashing point of an oil (i. e. the temperature at which it evolves vapour) may range.*

Such accidents as the explosion of a mixture of petroleum vapour and air in a ship on the Thames (the 'Maria Lee'), laden with about 300 barrels of petroleum and with other inflammable materials (in June, 1873), followed by her complete destruction by fire, in somewhat alarming proximity to the Purfleet powder magazines, and a similar accident in Glasgow Harbour, demonstrate the importance of the regulations which are imposed by the local harbour authorities, as directed by the existing Act of Parliament, upon all ships carrying petroleum spirit, with respect to their mooring and the landing of their cargo. The Act also gives to the municipal and other licensing authorities the very important power of including, as conditions of the licences, regulations with regard to the quantity of petroleum spirit to be stored in a particular locality; the nature and situation of the store with reference to inhabited buildings; the mode of storage, and the nature of the other goods to be stored with it. Very efficient regulations have consequently been adopted by the Metropolitan Board of Works—and some other local governing authorities—with regard to the stores themselves, the exclusion of any source of fire or artificial light from them, the nature of the receptacles in which the petroleum is kept, &c. The latter is a point of the utmost importance, as specially affecting the risk of accident in storing and *transporting* petroleum spirit, and one which it is very necessary that legislation should deal with more specifically than is at present the case. The existing Act specifies that limited quantities, for sale or consumption, may be kept without a licence, provided they are contained in small vessels of glass, earthenware, or metal, but it is silent on the subject of the vessels in which larger quantities are to be stored, or with respect to any regulations conducive to safety in the transport of petroleum spirit. As regards the first point, it certainly gives ample power to licensing authorities; but that legislation is urgently needed in regard to the second, was demonstrated by the recent fearful catastrophe on the Grand Junction Canal.

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* As the law at present stands, an oil, the flashing point of which is declared to be 99° by the official inspector, must be condemned; but another operator may make the flashing point of the same oil to be slightly above 100°. Practically, an oil with a flashing point of 97° or 98° would be quite as safe as one which answers to the test at 100°, in the hands of the same operator.

The liability of oil or spirit to leak from casks or barrels even of the best construction, consequent upon the rough usage to which these are unavoidably subjected when transferred from store to ship or carriage, and the reverse, need scarcely be pointed out. But even in the absence of leakage from the openings of the barrels, or from any accidental point of escape, evaporation or diffusion of the volatile petroleum will occur through the wood itself of which they are constructed, especially in the warm holds of ships or in stores exposed to the sun, even though the precautionary measure is frequently adopted of rinsing the barrel out before use with a solution of glue. It is evident that the object of imparting an impervious coating to the interior of the barrel can thus be only very imperfectly attained, and that, even if it were, the alternations of temperature to which the barrels must be exposed must in course of time open up places for escape by leakage or evaporation.

It is stated, on the authority of the manager of the extensive depôt for petroleum oil and spirit of the London Wharfing and Warehousing Company, at Plaistow, on the Thames, that in spite of the adoption of special arrangements for keeping down the temperature of the petroleum-spirit stores, whereby it is made not to exceed 62° Fahr. in the very hottest weather, the loss of spirit by leakage and evaporation is very considerable, amounting to an average of 18 per cent. As the loss on petroleum *oil* from leakage at the same establishment amounts to about 9 per cent., it is evident that the limpidity and volatility of the spirit give rise to a loss as great again as that which is simply due to leakage from imperfectly closed vents or defects developed by rough usage.

The dangers resulting from the escape of petroleum spirit or its vapour from receptacles in which it is kept, in confined spaces, where little or no ventilation exists, has been but too frequently exemplified by explosions more or less violent, followed by fires, in localities where it is stored or handled, or in the holds of vessels in which it is transported. Accidents of such kinds have been due either to carelessness in transferring petroleum from one vessel to another, in a shop or store in which a light has been burning at the time, or to a light being carried into or a match struck in a store where vapour has been escaping until it has formed an explosive mixture with the air. The explosion which occurred in a sewer at Greenwich last January, and was productive of much damage, was clearly traced to the entrance into the sewer of certain petroleum products (from a patent gas factory in the neighbourhood); the vapour from these had formed an explosive mixture with the air, which had become accidentally lighted, perhaps by the dropping of an inflamed pipe-light through one of the sewer-gratings. The speaker has a vivid recollection of an accident of this kind which he witnessed at the Royal College of Chemistry in 1847. Mr. C. B. Mansfield, who was then engaged in his important researches on the composition of coal-tar naphtha, which led a few years afterwards to his sad untimely death, was engaged at one extremity of a low room (38 feet long, about 30 feet wide, and 10 feet high) in converting one of the most important of these products—benzol—(which boils at 176° F.) into nitrobenzol in a capacious retort, which suddenly cracked, and, yielding to the pressure of its contents, allowed the warm liquid hydro-carbon to flow over the operating table. There was a gas-flame burning at the other extremity of the laboratory and no other source of fire. Within a very few minutes after the fracture of the vessel a sheet of flame flashed from the gas-flame along the upper part of the room and communicated to the table upon which the liquid had been spilled.

The firing of the ship 'Maria Lee' just now referred to was clearly proved to have been preceded by the explosion of a mixture of air and petroleum-vapour, produced by leakage or evaporation from the barrels stored in the hold of the vessel. The weather was very hot at the time, and, there having been a heavy thunderstorm the night before the fire, the hatches had been kept closed and covered over with tarpaulin, so that there was no possibility of ventilation. The vapour gradually diffused itself through the air in the ship during the night until the explosive mixture extended to the cabin at the after part. As the captain entered this cabin quite early in the morning (the *immediate* source of the fire was not clearly traced) a loud explosion occurred, and flame was immediately observed issuing from the fore part of the ship.

A similar explosion of petroleum-vapour and air occurred not long since in a ship at Glasgow; and lastly it was established by a sound chain of circumstantial evidence, that the explosion of the powder-laden barge in the Regent's Park must have been caused by the ignition, in the cabin of the barge, of an explosive mixture of air and of the vapour of petroleum, derived from the leakage of certain packages of the spirit which were packed along with the powder. The description given by an eye-witness, on board a barge in front of the ill-fated 'Tilbury,' of a bluish flash of flame, emanating from the cabin of the barge within a very brief period of the explosion, corresponds with the result produced when flame is applied to a mixture of hydro-carbon vapour with a large proportion of air. The manner in which the freight of the barge was enclosed by tarpaulins precluded ventilation, and was most favourable to the gradual diffusion of inflammable vapour, from even slight leakages in the barrels, through the confined air, until an explosive mixture extended to the opening in the bulkhead or partition, which in these barges separates the cabin from the hold.

It is impossible to protect heavy packages from rough usage, in the processes of unloading ships or other vehicles of conveyance; it is therefore most important that means should be adopted of thoroughly closing the vents of receptacles of petroleum spirit, by such means as are capable of sustaining ordinary rough usage without any injury to their efficiency, and that the improvement of the nature and construction of the receptacles themselves should be seriously considered with the view of reducing the liability to accidents resulting from the escape of the spirit or its vapours, and the consequent creation of danger connected with the transport and storage of these valuable illuminating materials.

The fact that combustible, and especially inflammable solid substances, if of sufficiently low specific gravity, and reduced to a sufficiently fine state of division to allow of their becoming and remaining for a time suspended in air, may furnish mixtures with the latter which partake of explosive character, scarcely needs to be pointed out. The ignition of a particle of such a substance, surrounded by atmospheric oxygen, will, under these conditions, at once communicate to others immediately adjacent to it; and if the particles of suspended solid matter be sufficiently numerous and finely divided, the ignition will spread throughout the mixture with a rapidity approaching that of a mixture of inflammable vapour and air, the development of gaseous products and heat being sufficiently rapid and considerable to produce explosive effects, which may even be of violent character, their violence being regulated by the nature and *inflammability* of the solid substance, the proportion and state of division in which it is distributed through the air, the quantity of the mixture, and the extent of its confinement.

Explosions of an accidental nature produced in this way are believed to have occurred in connection with operations in the chemical laboratory; but it was scarcely to be expected that the first clearly authenticated cases of any importance should have arisen out of the apparently harmless operation of grinding corn.

That a mixture of very fine flour and air will ignite with a flash when light is applied to it, and produce in a very mild form the species of explosion observed on applying a light to lycopodium suspended in air, is not very difficult of demonstration; but it is not easy to realize the possibility of the production of violent explosive effects by the ignition of such a mixture even upon a very large scale, though the rapidity of its ignition be accidentally favoured by the warmth of the atmosphere. Cotton mills have been known to be rapidly fired by the ignition of cotton particles suspended in the air; but, compared with flour, cotton is very combustible. Flour when absolutely dry would contain only about half its weight of carbon, and about 6 per cent. of hydrogen, the remainder consisting of nitrogen and mineral substances; constituents which, by absorbing heat instead of contributing to its development, must tend to reduce the rapid combustibility of the substance. Yet the possibility of very serious calamities arising out of the accidental ignition of a mixture of flour-dust and air has been but too conclusively demonstrated.

On the 9th July, 1872, the inhabitants of Glasgow were startled by an explosion which was heard to a considerable distance, and occurred in some very extensive steam flour-mills, of which the front and back walls were blown out, while the interior was reduced to ruins, and speedily enveloped in flame, destroying the remainder of the extensive buildings. Several persons were killed, and a number of others were severely burned, or injured by the fall of masonry. That the explosion was not occasioned by the steam-boilers employed as motive power in the mill was soon made clear; and by the evidence of persons employed in the mill at the time of the explosion, its origin was conclusively traced to the striking of fire by a pair of mill-stones, through the stopping of the "feed," or supply of grain to them, and the consequent friction of their bare surfaces against each other, the result being the ignition of the mixture of air and fine flour-dust surrounding the mill-stones.

This ignition alone would not suffice to develop any violent explosive effects; such ignitions, though occasionally observed in small mills, being caused either by the striking of fire by the stones, or by the incautious application of a light near the mill-stones, or the meal-spout attached thereto, have not in these instances been attended by any serious results. But in an extensive mill, where many pairs of stones may be at work at one time, each pair has a conduit attached to it, which leads to a common receptacle called an exhaust-box; into this the mixture of air and very fine flour-dust which surrounds the mill-stones is drawn by means of an exhaust-fan, sometimes aided by a system of air-blowers. The fine flour is allowed to deposit partially in this chamber or exhaust-box, and the air then passes into a second chamber, called a stive room, where a further quantity of dust is deposited. It follows that when the mill is at work, these chambers and the channels or spouts connecting them with the atmosphere immediately surrounding each mill-stone, are all filled with an inflammable mixture of the finest flour-dust and air, and that consequently the application of a light to any one of those channels, or the striking of fire by any one of the mill-stones, by igniting some portion of the inflammable mixture, will result in the exceedingly rapid spread of flame throughout the confined spaces which are charged with it, and will thus develop an explosion. The violence of such explosions depends much upon details of construction of the exhaust-boxes and stive rooms, and upon the dimensions of the channels of communication; it must obviously be regulated by the volume of inflammable mixture through which fire rapidly spreads, and upon the extent of its confinement. In the case of the catastrophe at Glasgow, the production of a blaze at a pair of mill-stones was observed to be followed by a crackling noise as the flame rapidly spread through the conduits leading to the exhaust-box upon an upper floor, and a loud report from that direction was almost immediately heard. Professors Rankine and Macadam, who carefully investigated the cause of this accident, report that other flour-mill explosions which they have inquired into had been observed to be attended by a similar succession of phenomena to those noticed upon this occasion. The bursting open of the exhaust-box by a similar though less violent explosion, attended by injury of workmen, the blowing out of windows and loosening of tiles, appears to have taken place on a previous occasion at these particular mills. In the last accident, however, the more violent explosion appears to have been followed by others, the flame having spread with great rapidity to distant parts of the mills through the many channels of communication in which the air was charged with inflammable dust, resulting from the cleansing and sifting operations carried on in different parts of the building, and rapidly diffused through the air by the shock and blast of the first explosion.

The subject of flour-mill explosions, though it has attracted little if any attention in this country previous to the Tradeston explosion, is discussed in Continental treatises on flour-mills, and the results of Messrs. Rankine and Macadam's inquiries have demonstrated that accidents of this kind are actually of ordinary occurrence in mills, especially since the introduction of the exhaust arrangements. Those gentlemen point out that it appears scarcely possible to guard against such accidents altogether, although the frequency of their occurrence may probably be much reduced by adopting efficient precautions to prevent, as far as possible, a stoppage of the "feed" to the mill-stones or the accidental introduction of nails between them together with the grain, and by prohibiting the employment of naked lights in the vicinity of the mills and the dust passages. In order to reduce as far as possible the damage and risk of sacrifice of life resulting from such explosions, it is important that all receptacles into which the dust-laden air is drawn from the mills, should be fixed outside the buildings, and constructed so as to offer as little resistance as possible to the sudden expansion resulting from the ignition of the inflammable mixture. The conduits leading from the mills to the exhaust-chambers should, moreover, be of small dimensions, and there should be no other communication between the interior of the building and the dust receptacles, which must not be opened while the mill is at work. By adopting precautions of this kind, the mill-owner may succeed, at any rate, in reducing the mischief resulting from an accidental ignition of flour-dust at the mill-stones to such limits that the mill itself and the lives of those engaged in it will not be endangered.

The production of explosions by mixtures of air with marsh-gas, coal-gas, petroleum vapours, or a finely divided inflammable solid, such as flour, has been shown to be due to the application of sufficient heat to some portion of the mixture to cause the atmospheric oxygen to combine with the combustible constituents of the gas, vapour or solid, the results being the development of chemical action, the formation of gaseous products, and their expansion by the heat developed. It need scarcely be said that the same explanation applies to the production of explosions by that class of so-called explosive agents which is prepared by intimately mixing combustible or inflammable solids with a solid oxidizing agent (i. e. an oxygen-compound which readily yields up a part or the whole of that gas under the influence of heat, and with the co-operation of chemical force, to carbon, hydrogen, or other readily oxidizable elements). Distinct from these explosive mixtures as regards their nature, but quite analogous to them in their behaviour and the effects they produce when subjected to heat or other disturbing influences, are explosive *compounds.* The majority of these contain carbon, hydrogen, and oxygen as the most important components; they are more or less susceptible of sudden or extremely rapid transformation into gases or vapours, attended by development of great heat, in consequence either of their resolution into their elementary constituents or, generally, of the rearrangement of these into comparatively simple forms of combination. Some of these explosive compounds are of such unstable character, that they are liable to undergo change from very slight inciting causes, such as the existence in them of minute quantities of foreign substances of active chemical character; or they may even be prone to absolutely spontaneous change. In such substances decomposition may be in the first instance established only to a very minute extent; but this decomposition, by the products to which it gives rise, and by the attendant development of heat, however small, may speedily promote further and more rapid change in the mass of the substance, so that eventually decomposition of violent nature may be established, and the principal portion of the compound may suddenly undergo the same transformation into gases or vapours, attended by the same development of heat, as though any one of the agencies (i. e. fire, friction, or percussion) ordinarily employed to determine the explosion of these bodies had been applied. Cases of so-called spontaneous explosion thus brought about are more familiar to scientific and manufacturing chemists than to the general public; but accidental explosions of very alarming and, in a few instances, of very calamitous character, are on record, which, though not actually of spontaneous nature, in the strict application of the term, have been brought about without any apparent application of external inciting agencies, and have hence, from a practical point of view, not been incorrectly classed as spontaneous explosions.

A few substances, well known to chemists, are so very unstable in character, or are so very difficult to prepare in a condition approaching purity, that they either begin to undergo change as soon as they have been produced, or very shortly afterwards, such change proceeding sometimes gradually and quietly until the substance has been transformed into non-explosive bodies, or occurring, in other instances, with a rapidity speedily resulting in the violent decomposition or explosion of the substance. Injuries more or less severe have been inflicted upon the discoverers or investigators of substances of this kind, or upon those who prepare them and exhibit their properties for instructional purposes; and such accidents occasionally occur, even though all possible or reasonable precautions appear to have been taken to guard against them. It has occasionally also happened that serious accidents have resulted from attempts to apply to practical purposes the explosive power of such substances (as, for example, the chloride of nitrogen and iodide of nitrogen,) by persons imperfectly acquainted with their properties or those of explosive substances generally. The great danger in which want of knowledge may involve experimenters in this direction is too obvious to need being dwelt upon.*

The risk of accident resulting from the liability of explosive compounds to so-called spontaneous decomposition has been on several occasions exemplified in the past history of the two most important of these compounds, guncotton and nitroglycerine. Until within the last few years it was believed that both these substances were in themselves so prone to change, that their preservation for any length of time without undergoing spontaneous decomposition, resulting most probably in explosion, was regarded as, at best, a great uncertainty. They acquired this character for instability partly from the imperfect purification of specimens prepared from time to time for scientific or instructional purposes, and partly from repeated attempts to utilize their explosive properties, and therefore to produce them in considerable quantities, while their nature and properties were still very imperfectly understood (this being specially the case with guncotton). The liquid nature and some other peculiar properties of nitroglycerine delayed any important attempts at its application for many years; on the other hand, guncotton presented to the superficial experimenter so many alluring advantages as an explosive agent that its discovery was very soon followed by its manufacture and storage upon a large scale, which speedily resulted in such serious disasters that the material acquired, within a very brief period of its discovery, an unmerited, but for a long time completely damnatory, reputation for utter untrustworthiness. The stability of properly purified guncotton, as well as that of nitroglycerine, has, however, now been for some time past fully established, and no difficulty exists in carrying on with safety their manufacture on such a scale as to satisfy the continually increasing demand for efficient preparations of these violent explosive agents. At the same time, the experience of the last few years has afforded repeated illustrations of the terrible risks and responsibilities incurred by manufacturers of these substances by the slightest departure from conditions essential to perfection and safety of manufacture, or by a relaxation of the strictest supervision in the production, purification, and storage of the materials. The calamitous explosion at Stowmarket in 1871, and many serious explosions at nitroglycerine (or dynamite) factories during the last few years, in different parts of the Continent, have demonstrated the liability of these substances to so-called spontaneous explosion, if the acids employed in their production are allowed to remain in contact with them for protracted periods, even in very small quantities. Any negligence whatever, therefore, in the production and purification of these substances, or the introduction, by accident or otherwise, of acid impurity into the finished materials, must in all probability prove destructive of their stability, and may lead to most serious disasters.

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* The great danger incurred in experimenting with, or attempting the manufacture upon a large scale, of substances of unstable and explosive nature, the properties of which are imperfectly understood, has been exemplified by repeated accidents with a liquid called methylic nitrate, the vapour of which is highly explosive. In 1872 a young English chemist of great promise lost his life by a very violent explosion which occurred in a laboratory in Germany, where he had been engaged in preparing large quantities of this substance with a view to its employment as a substitute for nitroglycerine preparations. No conjecture could be formed of the immediate cause of the explosion. Some time after a German manufacturing chemist of eminence lost the sight of one eye by the violent explosion of a very small quantity of the same substance while he was heating it in a glass tube; and in November last a very violent explosion occurred at the works of a manufacturer of dyes at St. Denis, in a building where very considerable quantities of the methylic nitrate were being prepared and purified for colour-making purposes. Two men were killed, and a number seriously injured. In this case the accident was clearly traced to an act of recklessness of one of the workmen. The use of lights was prohibited in the building where the purification of the substance was carried on, in consequence of the inflammable vapour evolved and the danger of explosion from its ignition. But on this occasion, a man not only brought a lamp into the room, but actually lowered it into a large boiler from which the liquid was being decanted with a siphon, his object being to see how much remained. The vapour in the boiler was at once fired, and a violent explosion occurred almost immediately, followed by a second, there being upwards of 1000 lb. of the material in an adjoining store.

In these respects the utilization of explosive compounds of this class involves special risks not attendant upon the manufacture of gunpowder and modifications of that substance; in others, however, it presents important elements of comparative safety. Thus, with proper precautions, the conversion of glycerine into nitroglycerine may be carried on with safety; the purification of the substance is not attended by any danger, and the manipulations attendant upon its conversion into suitable preparations for use (e. g. dynamite) are also in themselves perfectly safe operations. Again, the manufacture and purification of guncotton, and its conversion into the compressed or granulated substance, are absolutely safe operations, the material being wet throughout the entire course, and therefore quite uninflammable, until, when completed, it is dried by long exposure to air, or by artificial heat. On the other hand, gunpowder, and all preparations of similar nature, are explosive from the very commencement of their manufacture; the roughly mixed ingredients already furnish a material which deflagrates violently when a spark reaches it, and the subsequent operations of intimate mixture or incorporation, compression, granulation, &c., which are necessarily carried out with the nearly dry material, are all of a highly dangerous character, demanding for their safe performance the strictest attention to precautions which have been dictated by practical experience and knowledge of the sources of danger to be guarded against.

Accidents at gunpowder factories are very freqnent, and though they may not often involve considerable loss of life or destruction of property, the fact that their occurrence must in most instances be caused by partial, occasional, or complete and persistent neglect of precautions absolutely essential to the safety of the people employed in the works, or to a reduction of the risks of accident to the minimum, points to the necessity for improved legislation connected with manufactories of gunpowder and other explosive preparations, whereby the proper attention to regulations and precautions for safety may be rendered compulsory, and seconded by an efficient system of inspection. The absolute exclusion of lucifer matches from such works; the adoption of every possible precaution for excluding grit from all buildings in which the explosive substance is submitted to any kind of operation, by proper construction and closing of the buildings; the employment of special shoes and other external clothing for use in the buildings only; the total exclusion of iron, in any form, from the works; the proper separation from each other of the buildings in which the several operations are carried on, and their construction with a view to divert the force of a possible explosion into comparatively harmless directions; the reduction within particular limits of the quantity of explosive allowed to remain in any one building; the provision of efficient lightning conductors; —such are some of the principal precautionary measures which go far to reduce the possibility of accident in works of this kind (or the mischief accruing from such accidents when they do arise), and the adoption of which does not present any serious difficulty. And lastly, though properly first in importance, the manufacturers of gunpowder and other explosive agents should not only themselves possess some scientific as well as a practical knowledge of the nature and properties of the substances in the manufacture of which the lives of their workmen are at stake, but they also should ascertain and insist that at any rate the persons who act as managers and foremen in their factories should not be deficient in the elementary knowledge indispensable to a proper performance of their duties.

Major Majendie, the Government Inspector of Gunpowder Works, &c., has reported officially that he was "much struck, in the course of his inspections, with the extraordinary ignorance of even the most elementary dangers, and the precautions necessary for avoiding them, which prevails among persons in charge of important factories and magazines," and that there can be no doubt that to the ignorance and incompetence of such persons a large number of the accidents which occur are indirectly due. Surely it is in the interest of employers to adopt measures for securing that the management of their works is in the hands of competent men, experienced in the details of the manufacture, and possessing adequate general education and technical knowledge to fit them for posts of such responsibility. The obvious mode of securing this is to render it compulsory for such men to obtain certificates of competency before they can hold responsible appointments in manufactories of gunpowder and other explosive agents.

The manufacture of fireworks, ammunition, percussion caps, and other articles involving the application of explosive agents is, it need scarcely be stated, attended by liability to accidents similar to, and sometimes even greater than that existing in manufactories of gunpowder and materials of similar nature, and necessitates the adoption of precautions of the same nature as apply to these works.

Such necessity has, however, been very much disregarded in the arrangement and management of factories of this kind; and many very sad casualties have resulted either from utterly inadequate arrangements for localizing explosions and reducing them to small proportions, by regulating the quantities of material dealt with in one building, and sufficiently separating and subdividing the manufacturing operations, or from neglect of simple regulations for excluding sources of fire from the buildings. Thus, in inquiries following on explosions at ammunition or firework factories, it has been found in some instances a common practice for the men to wear their ordinary clothing when in the factory, and to carry lucifer matches about them. Again, at an ammunition factory, at Birmingham, where an explosion, not long since, resulted in the deaths of fifty-three women, it was brought to light during the inquest that there were open stoves, not even provided with fenders, in the middle of the sheds in which the powder-work was carried on; at these stoves the women used to cook their dinners, and they were also in the habit of shaking the grains of powder off their aprons into the stoves.

Other and special sources of danger exist sometimes in connection with this branch of the industry of explosive substances. Great stress has been laid upon the dangers which may arise in the manufacture of guncotton, and nitroglycerine preparations, from the liability of those substances to spontaneous change in consequence of the readiness with which their stability may be affected by imperfections in their manufacture. But there are several important instances of accidental explosions on record, which have occurred in the manufacture of pyrotechnic compositions and other articles of explosive nature in consequence of a liability to the establishment of chemical activity between the ingredients of such preparations by even very slight inciting causes. Thus, certain descriptions of coloured fires are readily susceptible of so-called spontaneous ignition or explosion, either simply from the unstable nature of one or other of their ingredients, or from so apparently trifling a cause as the absorption of a small amount of moisture, or the employment of a small quantity of an easily oxidizable oil or fat in connection with their application to pyrotechnic purposes. Two remarkable instances of unforeseen danger in dealing with materials of this kind, even when their stability, under normal conditions, is beyond question, have come under the notice of the speaker. In one case, an explosive mixture, prepared for use in percussion caps, was preserved in a moist condition with the view of storing it in a non-explosive form, but this very precaution gave rise to a serious explosion; chemical activity became established between the components of the mixture, through the agency of the water, attended by development of heat and the speedy production of violent chemical change, a result which would certainly not have occurred had the mixture been kept dry. In the other instance, some signal-lights, composed of a mixture of ingredients which long experience had shown to be in every way as permanent as those of gunpowder, were found to be undergoing decomposition to an extent which, had it not been noticed in time, must have resulted in serious consequences. The cause of this change baffled inquiry for some time, but ultimately it was clearly established that a very minute quantity of free acid contained in the paper linings of the cases in which the composition was confined (and derived from the antichlore used in the manufacture of the paper) had set up an action between the saltpetre and the orpiment composing this material, which spread gradually but with increasing rapidity through the highly compressed mass, being of course accelerated by the heat developed. Such occurrences afford instructive illustrations of the dangers which surround the manufacture and application of explosive substances, and of the necessity for combining continued study with untiring vigilance in dealing with these branches of manufacture.

Many of the most distressing accidents connected with firework-manufacture have occurred in dwelling houses of the lower class, situated in crowded districts, in which not unfrequently several families reside, and where people, generally in very poor circumstances, have carried on the manufacture of squibs, crackers, or pin-wheels, which they have either retailed, or generally supplied to more extensive firework makers or sellers. This production of fireworks has almost always been carried on illegally, i. e. without the maker having obtained the licence necessary, under an existing Act of Parliament, to enable him to manufacture fireworks; hence work of this kind has mostly been conducted as secretly as possible, other dwellers in the same house being often unaware of the dangerous operations carried on in the house itself, or sometimes in small sheds or out-houses. It can only be matter of surprise that firework accidents have not been more numerous, when it is remembered that the processes of mixing the firework compositions, ramming them into cases, &c., have frequently been carried on by these illegal makers in the common dwelling room, the several members of the family old and young taking part in the work, while cooking and even smoking may have been going on in the same room, and the work continued by candlelight in busy seasons, the powder or finished fireworks being placed in a cupboard in the room, or perhaps even in places affording less protection. This illegal firework making has been greatly checked of late by vigilance on behalf of the Government officials, and it is to be hoped that it will disappear almost entirely when new regulations are introduced which will facilitate the profitable employment of these small firework makers upon sufficiently extensive and properly organized premises of large manufactories, thus reducing the temptations which have hitherto existed for large vendors to purchase cheap fireworks of the poor people who carry on the manufacture illegally, and for the latter to convert their dwellings into sources of danger to themselves and their neighbours.

The fearful recklessness with which gunpowder and other explosive agents are handled and used by uneducated persons, such as these small firework makers, of which there are large numbers in the mining and manufacturing districts, and by the most extensive consumers of powder, namely, the miners and quarrymen, can scarcely be realized by anyone who has not had opportunity to acquire by personal observation a knowledge of the state of things. A miner may be seen with his naked lamp or tallow candle fixed in his hat or plastered against the rock close to the blast-hole he is about to load, pouring the powder into his rough measure, or his cartridge case, from the flask produced from a pocket which is often also the receptacle of lucifer matches, and at times of a half-finished pipe of tobacco (if he has not the pipe actually alight in his mouth). Having inserted the charge into the hole, he will proceed with the operation of *tamping*, which consists in ramming débris of the rock into the hole as tightly as possible by means of a heavy iron tool and a hammer. Grains of powder lying upon the sides of the hole, and sometimes forming a train to the charge at the bottom, are thus frequently submitted to most violent friction, and it is wonderful that accidents in this process of loading are not more frequent; but when, in spite of oft-repeated cautioning, the miner proceeds to submit to the same, and sometimes even to more violent, treatment a cartridge of guncotton, dynamite, or other explosive agent ignited more readily than gunpowder by friction (and which is tightly confined in the blast-hole by the act of ramming with a heavy tool), it is not a matter of surprise that fatal accidents should occur during the employment of these substances by the miner, although he is exposed to less danger with them when carrying them about his person or handling them for actual use, because they are not violently explosive in small quantities when unconfined.

It is, however, more particularly from the fact that there are no regulations forbidding or restricting the making up, in dwelling houses, of blasting cartridges, mining fuzes and the so-called powder-straws used in blasting, that the chief liability to accidental explosions in mining districts arises. Miners are constantly in the habit of keeping considerable quantities of powder in their dwelling rooms, and making up their cartridges or fuzes (straws) at night. In a recent official report the Head Constable of Middlesborough stated, "In one case, at Eston, the roof of a house was blown off, and on inquiries being made respecting it the man . . . . stated that he was tallowing the end of the straw, when he put it too near the candle flame, which ignited the straw; he threw the straw away and it fell into the barrel of powder, which exploded." The Head Constable of Richmond in Yorkshire reported as follows: "I have seen miners with a quarter cask (25 lb.) making their charges by the fire frequently, and they generally keep it under the bed or in the pantry, where their children have access to it." At Wigan a collier was filling cartridges from a can of powder, when a spark from the candle fell into the can and produced a serious explosion. In another instance a boy was left alone in a house where an open barrel containing 7 lb. of powder, with a piece of wood simply placed over the head, was in the bedroom. A box of matches was known to be in the room, and it is supposed that the boy, who died from injuries received, exploded the powder by means of them.

Numerous illustrations such as these could be quoted of the fearfully careless manner in which powder is very generally dealt with in manufacturing districts. It naturally follows that other explosive agents, such as dynamite and guncotton, should be treated with similar and perhaps even greater recklessness. The apparently less dangerous nature of such materials when unconfined tends to render the miner even more regardless of precautions, and hence it is unquestionably wrong to foster the notion of the safety of these materials in the hands of the miner, especially as it frequently occurs that the men who use these materials are unable to read the printed instructions which are supplied by the manufacturers with the cartridges for the purpose of guarding against accident. Thus several frightful accidents have occurred through direct exposure to the fire of frozen cartridges of dynamite for the purpose of thawing them,* and the apparently harmless nature of dynamite and compressed guncotton has on more than one occasion caused the miner, when a charge has become jammed in a blast-hole, to endeavour to drive it home with his rammer, with all the force at his command, with what result need scarcely be stated.* The wholesome effect of stringent regulations which keep constantly before the minds of the ordinary operative the dangerous nature of gunpowder and all operations connected with it and with other explosives, is well exemplified by the care and caution which generally become developed as characteristics in the men employed in Government factories of this class; and although there is but little hope of the possibility of enforcing even the mildest precautionary measures with the miners themselves, it can hardly be doubted that their minds must become to some extent influenced by example, and that, by constantly witnessing the strict enforcement of precautions at the stores to which they resort to obtain their supplies, they must in time become impressed with the necessity for the exercise of some amount of care in handling explosive substances.

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* The necessity for thawing dynamite cartridges before use in cold weather, and the disregard of instructions furnished by the makers for doing this with safety, constitute one of the chief causes of accidents with this material. Fatal explosions have resulted from the placing of cartridges in front of a fierce fire or upon a stove. Quite recently a pit sinker, at Mountain Ash, placed some dynamite in the oven of the fireplace in his house and sat down to breakfast. Before the meal was ended the dynamite exploded, killing the man and injuring two others.

It does not admit of dispute, however, that the recklessness of the miner has actually been fostered hitherto by the utter disregard of all ordinary precautions which they must but too frequently witness at the stores where the powder is sold or issued to them. The practices of small dealers in gunpowder present illustrations of ignorance and recklessness if anything even more appalling than those which the habits of the miners furnish. The manner in which powder is often dealt with by those in charge of the stores or magazines in quarries or mines, and who have to issue supplies to the men, may be illustrated by one or two examples from a report to the Home Office by Major Majendie. At a quarry in Scotland he was conducted by the man in charge to a magazine containing about 6 cwt. of powder. For this purpose the man lit a naked candle; as they entered the door it was blown out by the draught, but the man produced some matches from his pocket and rekindled it. When asked how he issued the powder to the men, he held the candle with his left hand, and ladled the powder out with the other hand. The practice of smashing in the head of a powder barrel with an iron crow-bar is stated to be a very common one, or else a large stone is used, or a hole is bored into the head with a steel bit; and as an extreme instance of recklessness the case of a man is quoted who was in the habit of boring into the barrels with a red-hot poker; on one occasion, the lid of the barrel being thinner than usual, the heated iron was thrust into the contents of the barrel, and the man fell a victim to his very original mode of dealing with packages of gunpowder.

In some mining districts it has been customary to pay no regard whatever to the suitability, in point of safety, of the localities selected for the storage of powder. It has not unfrequently been kept in large quantities (e.g. 500 lb.) in ordinary buildings, quite close to dwelling houses. An instance is quoted in an official report by a chief constable, of about 3 cwt. of powder being kept in a colliery lodge constantly used by workmen, and in which a fire was regularly kept. Even where magazines have been provided, in connection with extensive mines and quarries, many instances are on record of gross ignorance or carelessness in regard to the precautions essential to the safe handling of gunpowder. The men who go to the magazines for supplies of powder, and even the man who is in charge, are allowed to enter the building in their iron-shod dirt-covered boots, often coming to the store smoking, the pipe being merely put into the waistcoat pocket when they get close to the building; the barrels containing loose powder are often left unheaded, and powder grains lie upon the floor; the door of the magazine in some instances opens inwards, scraping the floor as it is pushed open. A magazine in which this utter want of precautions was observed in the most extreme degree contained several tons of gunpowder. In illustration of the way in which explosions occur at such stores, it may be mentioned that some children saw some loose powder outside a store (in Cornwall), and set fire to it; the contents of the building were thus exploded, there being a train of powder grains leading under the door to the stock of powder in the building.

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* A fatal accident occurred quite recently at Ebbw Vale in consequence of a man using great force in ramming a dynamite charge into a blast-hole with a wooden rammer. The printed instructions distinctly laid down that the cartridges were never to be rammed. It came out at the inquest that the partner of the man killed could not read.

The strenuous exertions of the Government inspectors during the last few years have already resulted in a considerable amelioration of this lamentable condition of things, although the existing state of the law affords them little power to enforce simple regulations which are vital to the safety of the people employed, and often of the neighbourhood, but scant regard being but too frequently paid to the position of even extensive stores or magazines with reference to contiguous habitations.

The fearful explosion which occurred at a gunpowder merchant's magazines at Erith, containing 51 tons of powder, in October, 1864, and the explosion at Liverpool, in the same year, of the ship 'Lottie Sleigh,' laden with 11½ tons of powder (through the accidental spilling and ignition of some paraffin oil), directed public attention at that time to the existing laws relating to explosives just as forcibly as did the recent explosion in London, and their very defective condition was then, as now, apparent. No steps have been taken since that time, in the way of legislation, to afford the public greater protection from dangers arising out of the storage and transport of gunpowder, or to bring the doings of powder makers and consumers more under legal control, although the Government did in 1865 contemplate for a time the enactment of new laws relating to explosives.

Very considerable alarm was felt then, as lately again, at the possibility of most calamitous disasters arising out of the collection of very large stores of gunpowder, by powder merchants as well as by the Government, in particular localities, some of them in close proximity to populous districts. In some instances representations to the Home Office, which were followed by efficient inspection, made at the instance of Government by Colonel Boxer, led to the closing of extensive merchants' magazines existing in objectionable positions, or to a considerable reduction in the amount of powder stored in them; but it does not appear that any substantial improvement occurred in the extent to which precautions, vital to the safety of the magazines, were observed or attempted to be carried out by persons in charge of them.

The subdivision of the reserves of gunpowder, which it is the duty of the Government to maintain to as great an extent as is compatible with efficient arrangements of inspection, and with the exigencies of the service in time of war, is unquestionably most advisable. It is admitted to be desirable that the magnitude of depôts of powder at some of the principal establishments should be reduced in extent, and in some instances extensive depôts which existed in close proximity to towns have been removed or reduced to insignificant proportions. It must be borne in mind, however, that the possibility of the explosion of magazines, where powder is simply stored as a reserve, emanates almost exclusively from causes external to the depôts themselves; and that as long as these are situated at such distances from habitations as will secure them against fire reaching any part, the precautions adopted by Government to guard against accidents from electric discharges, and against the possibility of any danger arising within the establishments themselves, render such depôts quite safe, provided operations connected with gunpowder (such as the making up or breaking up of cartridges) are not carried on at or in close proximity to such magazines. It is only in conducting manipulations with gunpowder that danger may arise (from any causes excepting fire or electric discharges); and therefore any operations with powder, such as the making up of cartridges, or any transactions involving the frequent issue and receipt of powder, or the opening or repacking of powder packages, should be imperatively conducted in establishments distinct from those simply devoted to the preservation of reserve supplies.

The recent powder explosion revived the discussion which arose some years ago as to the probable advantages to be derived, in point of safety, from a dilution of gunpowder, for purposes of storage and transport, with some incombustible, non-conducting material, which, by isolating the grains from each other, would prevent the transmission of fire from one to those surrounding it. Although quantities of gunpowder may thus be rendered practically non-explosive, there can be no doubt that the possibilities of accident must be considerably increased by the additional manipulations to which the powder must be subjected in the application of such a safeguard. If, as in the case of compressed guncotton, gunpowder could be preserved in a sufficiently wet condition to be perfectly uninflammable, the drying, as a [illegible] manufacturing operation, being simply deferred until the material quired for use, the protection afforded by a diluent could be without the introduction of any extra operations involving, adoption, the usual risks invariably attendant upon manipulations with an explosive. The impracticability of such a course need however be hardly pointed out; in this particular respect, therefore, guncotton possesses a decided superiority over gunpowder, especially as for almost every purpose to which it can be advantageously applied it may be employed in the wet perfectly uninflammable condition as effectively as if perfectly dry, by the adoption of very simple modes of exploding it.* Supplies of guncotton in the condition in which it can actually be used without further preparation may therefore be stored without the precautionary measures indispensable in the case of gunpowder.

Experiments on a considerable scale made by the late Government Committee on guncotton, &c., demonstrated conclusively that when stored in *small* quantities (two or three hundred pounds) dynamite and dry guncotton are unquestionably much less dangerous in their character than gunpowder. Such quantities, even if confined in strong boxes and contained in buildings filled with inflammable matter, may burn away entirely, when these are set on fire, without developing explosion; but the burning of 5 to 6 cwt. of these materials under similar conditions may terminate in an explosion, unless they be less strongly confined; and there is no doubt that very considerably larger quantities, if set fire to, will not even need other confinement, to develop an explosion, than that to which the interior of the mass of explosive is subjected by the external portions, and by the pressure of gas developed by the fierce burning of the substance. The degree of comparative safety with which explosive materials of this description may be stored is therefore regulated by the quantity and the extent of confinement, and hence great caution must be exercised in relaxing in their favour the precautions and restrictions applied to gunpowder. Indeed, the prudence of relaxing them at all is questionable, as the result may be the fostering a sense of security in those who have to use these substances, which, by encouraging negligence and forgetfulness as to the properties of the materials dealt with, may be productive of disastrous results.

A regulation of the quantity of an explosive stored in one place, and of the mode of storage, by the nature of the particular material, and the circumstances which may tend to develop its violent explosion, would involve a combination of complex rules with an impractically comprehensive system of supervision; and it is therefore obviously not only wise but also necessary to deal with the storage, upon an extensive scale, of all explosive substances upon the broad general basis of their being susceptible of violent explosion from accidental causes; any relaxation in favour of the comparatively safe materials being suffered only to apply to small quantities.

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* In experiments recently conducted by the late Guncotton Committee at Eastbourne, two separate tons of compressed guncotton, in the wet condition in which it is now stored by Government, were exposed to fierce fires in strong magazines, the guncotton being confined in stout boxes and in wooden tanks. No explosion occurred; the guncotton was slowly converted into gases and vapour.

The safe *transport* of gunpowder and other explosive agents demands the application not only of many of the precautionary measures which should be observed in their manufacture and storage, but also of certain others of special character, to meet the liability to casualties arising out of the ordinary accidents of locomotion, and out of the more or less unavoidable exposure of the explosive agents during transport to treatment and conditions involving risk of accident. Thus the submission of packages of gunpowder and many other explosive substances to the continued concussion unavoidable in transit by road or rail necessitates the employment of strong and thoroughly sound or tight receptacles, so as to prevent the escape of fine particles or dust from the packages into the conveyances, and the possibilities of accident resulting therefrom. Again, the transmission of both large and small quantities of explosives by land or water must be governed by well-defined rules regarding the extent and manner which the transport of such materials should be kept distinct from that of other goods, the nature and construction of the conveyances specially devoted to, or occasionally selected for, their transport, and the conditions generally under which such transport shall be accomplished.

The utter inadequacy of the existing regulations as to the conveyance of powder, &c., by land or water, and the flagrant manner in which even these defective regulations are but too frequently disregarded, are matters to which public attention has been much directed since the explosion in October last, and which are but in harmony with the negligence and ignorance displayed to so alarming an extent in connection with the handling and storage of gunpowder. Thus, the packages (barrels, &c.) in which powder is transmitted to distant places are often so imperfectly constructed that the grains escape into the cart, or the hold of a vessel in which the powder is stowed, where they may become mixed up with grit and be eventually trampled upon. As regards the vehicles in which the powder is transported, some regulations exist with respect to the employment of covered or uncovered carts with reference to quantities of powder exceeding considerable limits; but there is no law requiring carts or barges to be specially constructed or employed so as to exclude sources of danger (such as grit, employment of iron fastenings, &c.). In the mining districts and even in towns powder is constantly conveyed in dangerous quantities in ordinary carts, which may have been used for carrying stones, coal, or road rubbish. It may be and frequently is conveyed in carts with other goods, such even as lucifer matches and petroleum; there is no regulation to prevent the person in charge from smoking while in his cart, or stopping at a public house, leaving the powder standing at the door, and Major Majendie reports that such occurrences are frequent.

Public conveyances often carry packages of powder, without any precautions, other than perhaps a caution to a passenger who may unwittingly be exposing himself and companions to immediate danger. Thus there is a case officially recorded of a man who, on getting on to an omnibus at Wigan, was requested to put out his pipe, as the passenger next to him had 100 lb. of powder in a barrel beside him. The conveyance of large quantities of gunpowder in a number of wagons passing close together through towns has been no uncommon occurrence; thus Major Majendie discovered that it was a practice in Edinburgh to convey as much as twenty tons and upwards of powder at one time through the streets, the public having thereby incurred the greatest possible risks, through smoking going on while the carts have been standing in crowded places. A similarly extensive conveyance of powder through the most populous districts of London for transfer to ships at wharves has often taken place; and as one of many instances mentioned by the police authorities may be quoted that of a train of fifteen vehicles loaded with about twenty tons of gunpowder passing from Camden Town to Blackwall Stairs (a densely populated place) to be shipped off, the whole of the loaded vans being detained in the street for some time for the arrival of the barge to which the consignment was to be transferred. Railway companies as a rule adopt comparatively efficient precautions with respect to the transport of powder and explosives. The former is conveyed, when in sufficient quantities, in vans specially constructed, and more or less well adapted to the purpose, or, when the quantities are comparatively small, in barrels separately packed in specially constructed metal cylinders. With regard to the conveyance of other explosives and of small packages of fireworks, the regulations and charges adopted are in most instances so restrictive, that they lead to much surreptitious carriage of such goods, whereby passenger and goods trains are often exposed to great risk.

The carriage of powder and other explosives by water should be the safest mode of conveyance, if proper regulations could be enforced; but here again the Regent's Park catastrophe has afforded a terrible example of the danger to which the public has been continually exposed, by the profound oblivion which appears to have reigned in the minds of those engaged in the transport of powder by water-traffic as to the explosive nature of this material. The arrangements made by Government for the transfer of gunpowder by barges from the factory to the depôts, illustrate the nature of the precautions which are regarded as indispensably necessary by those who have well considered the subject. No iron whatever is allowed to enter into the construction of the barges; the men engaged in stowing the powder wear special shoes and clothing; every particle of grit or dust is most scrupulously removed before the powder packages are introduced, and these are very carefully and securely covered in. The bargemen are strictly prohibited from smoking and carrying lucifer matches either about them or in any part of the vessel; no fire is permitted on board during the loading, carriage, and unloading of the powder, and the strictest regulations exist with regard to the manner in which the journeys are to be performed. The barges are, moreover, provided with valve-arrangements by means of which they may be sunk in a very short space of time, should danger of fire arise from neighbouring barges or other sources.

In other vessels used occasionally for the transport of powder by Government the foregoing precautions are carried out as far as possible; the vessels are not specially constructed, but, in order to exclude all iron fastenings and accidental grit from contact with the powder barrels, hides and thick woollen covers are spread over the bottom and sides of the vessel. Although the barges of canal owners and others are subjected to this practice when carrying powder for Government, it has not been so with regard to the freights of powder carried for merchants; and regulations respecting abstinence from smoking, or exclusion of fires or matches, have been avowedly of the most lax description, or have even become a dead letter, although the existing law is thus set at defiance. The practice of carrying lucifer matches about the person, and of smoking, has unquestionably been among the most prolific sources of accidents connected with gunpowder, and it need scarcely be stated that they are almost the most important dangers to be guarded against in the transport of powder in barges. During the inquest which followed the great explosion at Erith in 1864, it transpired that lucifer matches were allowed, as a rule, on board the barges and vessels carrying merchants' gunpowder; and while one powder barge was unloading, another empty one by its side was seen to have a fire on board. The falling of a match from a man's pocket while he was engaged, in a stooping attitude, in moving the powder packages from the barge to the magazine was considered by Colonel Boxer, after careful investigation, to have been the most probable cause of that explosion.

Another source of danger, which, viewed in conjunction with the non-exclusion of sources of fire, is of a most alarming nature (and which has evidently been of common occurrence), is the carriage of gunpowder in barges as part of a miscellaneous cargo. There are obviously many goods which may be transported together with powder without danger to the latter, although the special care which gunpowder should always receive renders its isolation from other goods desirable under all circumstances. Recent experience has, however, demonstrated that it has not been the custom to exercise any discrimination in the stowage of packages of gunpowder with other goods, when the former was insufficient in quantity to constitute a barge load. It is scarcely to be realized that the indifference with which gunpowder has been treated by those who undertake its transport by water could have attained such an extreme that powder packages could be stored in the hold of a barge side by side with casks of petroleum spirit, by persons whose experience must have made them cognizant of the liability to leakage of petroleum from such casks. Such leakage (occasioned perhaps by rough handling in placing it on board) must inevitably furnish in course of time an explosive atmosphere by the diffusion of inflammable vapour through the air confined in the barge-hold (which is closely covered in); the extension of this explosive mixture to the small opening which these barges contain in the bulkhead separating the hold from the small cabin, or its penetration through crevices in the bulkhead, is but an affair of time; and then, whichever of the several sources of fire provided on board the barge, i. e. the stove, lamp, or lucifer matches, happens to come within reach of the quickmatch which the explosive atmosphere constitutes, completes the arrangement for inflaming any small quantity of petroleum which may have leaked out on, or in close proximity to, a powder barrel. But for the fact that a concurrence of several conditions is essential to the communication of fire to gunpowder, through the agency of a leakage from a petroleum cask in a confined space, it can scarcely be doubted that the rude awakening which the public recently received, to the danger they were frequently exposed to in the vicinity of canals where powder traffic goes on, must have occurred long since. The simple flash produced by the ignition of a mixture of hydro-carbon vapour and air would probably not suffice to ignite powder grains exposed to it; but any small quantity of the liquid itself which, leaking from a cask, has furnished the vapour, may be in close proximity to a few grains of loose powder, or upon a powder barrel which is not securely closed, or some other simple conditions resulting in the conveyance of the fire to the powder may be fulfilled, and then explosion must ensue.

The imperative necessity for better legislation in reference to the transport of powder has been so convincingly demonstrated by the Regent's Park disaster, that the long-contemplated revision of the law relating to explosive agents would, there is little doubt, have been hastened thereby had the serious attention of the Government not already been devoted to this subject. During the first two years which succeeded the appointment of Government Inspectors of Gunpowder Factories and Magazines, the unceasing labours of Major Majendie resulted in so convincing a demonstration of the utter inadequacy of the existing laws relating to the manufacture, storage, transport, and use of gunpowder, and other explosive agents, to afford protection to the public and to those dealing with these substances, that the intention (already entertained by the Government in 1865) of framing a bill to amend the Gunpowder Act was actively pursued by the late Ministry in 1873. Although the submission to Parliament of new measures was delayed by the change of Government, this delay has been productive of benefit, inasmuch as the present Government has secured most important aid in the preparation of the comprehensive bill which has recently been submitted to Parliament, from the labours of a Select Committee, appointed last session, to inquire into the working of the existing laws, and the directions which fresh legislation should take.

The rapid development which has taken place in the manufacture and use of other valuable explosive agents has considerably increased the necessity for comprehensive measures regulating these important branches of industry. When the introduction of nitroglycerine into this country, a few years ago, was speedily followed by numerous deplorable disasters, the protection of the public demanded the imposition for a time of severe restrictions in dealing with an explosive agent of most violent character, the nature of which was still very imperfectly understood. A measure was therefore made law in 1869, whereby the employment of nitroglycerine itself and of its preparations was placed entirely under the control of the Government. Although this arrangement involved great labour on the part of the Government officials, and placed what now certainly appear to be unduly severe restrictions upon the manufacture and employment of dynamite, its necessity and beneficial operation as a temporary measure have been fully admitted by those chiefly interested in the progress of nitroglycerine preparations; and there is no question that it importantly promoted the *wholesome* development of the application of dynamite in Great Britain, where it has during the last three years secured a firm footing as a material indispensable to mining industry. The existing laws relating to nitroglycerine preparations can now be modified with great advantage, and no important difficulty should be experienced in including in a general Act the measures necessary for developing and regulating, with proper security to the public, the production of this and other valuable substitutes for gunpowder, in such a manner as not to hamper the powder industry with undue restrictions. Other long-established branches of manufacture connected with explosive materials, which are quite as dangerous as many of new creation, but have enjoyed comparative immunity from restrictions, will also be brought under the operation of that systematic supervision which the security of the public demands.

It is obvious that to attempt to deal in detail in one, or more than one Act of Parliament with the various modifications of regulations specially applying to gunpowder, which the peculiarities of other explosive agents, or of branches of manufacture involving risk of explosions, necessitate, would be to overburden the law with an unwieldy mass of measures which, however circumstantial in their nature, would certainly fail to embrace all conditions and contingencies likely to arise. Hence the wisdom (which has been so clearly demonstrated by the Nitroglycerine Act) of entrusting the Government with discretionary powers, under the operation of a comparatively simple but sufficiently comprehensive Act of Parliament, can scarcely admit of question.

The beneficial results attainable by a systematic and thoroughly authoritative supervision, by Government inspectors, of factories and stores of explosive agents, if conducted with intelligence and discretion, have been most convincingly demonstrated by the great good which it is admitted on all sides that the inspectors have already succeeded in accomplishing, even with the very insufficient powers which the present state of the law affords them. The favourite argument of some, that Government inspection must operate mischievously, by diminishing private responsibility, has certainly received no support from the results of inspection, so far as the experiment has been tried. It will scarcely be asserted that a manufacturer or store-holder who may have willingly adopted, as suggestions which the inspector has no power to enforce, measures conducive to the safety of life and property, would be careless in the application of those measures because their adoption was no longer optional, or because the responsibility for their due observance was to some extent shared by the inspector. This very system of inspection cannot fail to benefit those interested in different branches of the industry of explosives by reducing the necessity for hard and fast rules with respect to the arrangement and conduct of works, which might in many instances entail hardship or inconvenience without any real necessity, and by strengthening the hands of factory owners, and thus rendering comparatively easy the proper observance and enforcement of regulations for the safety of the men and the works. It is, however, especially in connection with the storage, transport, and employment of gunpowder and other explosives in mining districts that efficient inspection, supported by the reasonable power which a well-considered Act of Parliament cannot fail to afford, may be confidently expected to produce important beneficial results, not the least of which will probably be the wholesome influence exercised indirectly, by the force of example, upon the miner or pitman, whose ignorance has fostered the indifference with which long habit has led him to regard the possibility of danger.

But although improved legislation, and the beneficial regulations thus supplied, may be confidently hoped to effect an important reduction in the number and magnitude of the disasters now recorded as accidental explosions, it would obviously be worse than shortsighted to encourage a reliance upon legislation alone as a safeguard against the evils which lead to casualties of this kind. Punishments inflicted for transgression of the law may engender caution, but the disasters which arise from ignorance are not likely to be importantly reduced in number by legislative enactments alone.

It is to the general promotion of education among the people, and to the spread of scientific and technical knowledge, if even of the most elementary kind, among employers and employed, that we must look for a substantial diminution of these casualties which the uneducated mind is but too prone to attribute to accident, and the prevention of which rests, at any rate to a large extent, with those who are at present tacitly content to regard them as inevitable.

[F. A. A.]

## WEEKLY EVENING MEETING,

Friday, March 19, 1875.

SIR FREDERICK POLLOCK, Bart. M.A. Vice-President, in the Chair.

RICHARD LIEBREICH, M.D. M.R.C.S. M.R.I.

### *On the Real and Ideal in Portraiture.*

GREEK sculpture, arriving at its highest development, fixed for the representation of the deities types of ideal beauty. Ever since, sculptors have divided themselves into such as followed those laws of beauty derived from the study of the classic works of art, and such as worked after nature, putting the truth derived from it above the perfect beauty. In our times certain subdivisions have been observed, forming various shades within each of these two schools, without, however, effecting a real amalgamation of the two different tendencies. Thus, in the idealistic school some have strictly adhered to the antique; others, though making the study of the antique their starting-point, have yet in their compositions made concessions to our modern feelings. On the other hand, among the realists, some, though guided by the direct observation of nature, have yet adhered to the principles of antique sculpture as far as the choice and representation of the subject are concerned, whilst others have preferred the exact copying of actualities.

In painting, idealists and realists are certainly as far apart as in sculpture. There exist, however, in the different individualities of the artists almost all the transitions from one tendency to the other.

I shall therefore keep principally to sculpture, when trying to analyse the question of the real and ideal in portraiture. Among the Greeks the portrait, as representing the real, formed originally the contrast to the ideal creations of the types of the deities. It was only much later that in the portrait also the contrast showed itself distinctly between real and ideal; and especially so from the time when, under the successors of Alexander the Great, Lysistratus came to the realistic extreme, and made casts after nature, filling them with wax and retouching them afterwards.

In the Roman period the realistic portrait developed itself to such a perfection as still to give to this branch of art value and signification, when sculpture in general was rapidly approaching degeneration. The contrast between the idealistic and realistic portrait in this period is principally based upon the purpose of the representation. The custom of representing as deities the emperor and his family, his friends, the higher functionaries, and even private persons of no merit, led to the imitation of the ideal types of the deities, with a face more or less approaching this ideal type. To this the purely iconic realistic bust, showing the individual in his real aspect, originally formed a sharp contrast, though occasionally a sort of confusion was produced by a realistic head being placed on an idealistic body.

Though no such reason any longer exists in our time for so sharp a separation, yet the separation itself still exists. Upon what conditions, then, is it based? This we shall try to show by examples, and in order to enable us to do so, we must above all see these examples in a proper light, on the importance of which I should like to make a few observations.

The importance of the background and light for the impression to be produced by pictures is generally acknowledged; it appears however that the public are not so generally conscious of the fact that the background is quite as important for sculpture, and that for it the right light becomes a vital question. Otherwise an improvement in the system of exhibiting classic works of art in museums would be insisted upon; and in private collections the places assigned to sculptures would not be allowed to depend upon accidental circumstances. Thus in the Louvre we see its jewel, the Venus of Milo, placed in a bad light. In the South Kensington Museum, the highly interesting busts of the Florentine School of the fifteenth century are placed in such a manner that the height of their position, the colour of the background, and the light falling upon them, render it all but impossible to examine them, whilst very trivial objects have received the best position and light. In the National Portrait Gallery, the busts are set on shelves 9 feet from the floor and in front of the windows; so that they can only be seen from below, while they are lighted from the front and from below. Among the faulty arrangements for exhibiting sculpture we must mention also that of the Royal Academy. It will certainly be improved, as soon as the public begin to take an interest in this matter and to understand it. For this purpose it is only necessary to afford an opportunity of seeing a well-arranged exhibition. I convinced myself of the simple means by which such an arrangement may be effected, when visiting the exhibition in Milan in September last. We have attempted to give you an idea of the Italian arrangement, of which you will find an imitation in the Library. Several distinguished artists have kindly sent in some of their works for this purpose, and it will surely afford you a truly artistic enjoyment to see them after the lecture. There you will receive the general impression of sculpture exhibited in the right light: here we shall analyse the details of this effect, and that with reference to the face chiefly.

The absolute dependence of sculpture on the light that falls upon it might be considered a defect in the art, and confirm the not infrequent assertion that sculpture is altogether unfit for portraiture. In order to be perfectly just let us enter into the argument at once. The painter, the draughtsman, the photographer, if about to make a portrait, carefully select the right light, being convinced that to a great extent the success of the portrait depends upon this. As soon as they have fixed upon the effect of light which they wish to produce in their picture, this effect will remain substantially the same under all circumstances, although the general impression of an oil painting may be slightly modified according to the light which falls on it and according to its surroundings. The sculptor, on the contrary, shapes a body which may be exposed to all the various effects of light to which the original might be exposed, and amongst others to all those under which the painter would declare it impossible to paint the original. What painter, for instance, would consent to paint a portrait with the light coming from the front and from below? The effects of light and shade characteristic for the human face disappear completely under such circumstances. Let anyone cover a human face with white paint and throw a light upon it from the front and from below; then it will be found that the features disappear completely; yet marble busts are frequently exposed to this kind of light.

The reason why the human face appears to advantage only when light is thrown upon it more or less from above, and why its likeness can be correct only under such a light, is one very deeply rooted. The nature of man, his erect bearing, the direction of his look, everything that in his appearance distinguishes him from the animal bent to the earth, necessitates that conformation of the forehead, the nose, the mouth, &c., which is characteristic of the human face, and which was required by reason of the sunlight coming to him chiefly from above. And therefore, also, it is only the light coming more or less from above that shows the forms of his features in a characteristic way. Thus, it is no exorbitant demand that sculpture ought to be seen in the same light in which the original appears to the best advantage, and which every painter is free to choose for himself and fix in his picture.

In the plastic reproduction of the body, the coarser and more general forms may be seen with tolerable distinctness even when a false light falls upon them, as the stereoscopic view aids the spectator. In the case of the more subtle features of the face, however, the stereoscopic impression plays but a subordinate part, on account of the slight difference of depth of the characteristic features. The true impression of the face in a bust therefore depends almost exclusively upon the play of light and shade which gives to the one-coloured material the intended aspect. And this light and shade will be judiciously disposed only if the portrait is looked at as nearly as possible from the direction and in the light in which the artist intended it to be seen.

A closer examination of the technical details will convince us that this does not only apply to works of sculpture which specially aim at a picturesque effect, but also to those which keep strictly to the form. The more elaborate the modelling, the more expressive the features, the richer the detail in the bust, the more it has to gain by a correct light and to lose by a false one. That is the reason why, in exhibitions arranged without due consideration to light, we find that levelling influence which places in the same range the best and the worst, and thus gives the finishing stroke to that infinitely wearisome impression produced by the pale marble heads placed one near the other on a shelf, which the spectator passes as quickly as possible in order to get to the more attractive picture gallery of the Royal Academy.

Let us now compare, with correct and with false light successively, one realistic and one so-called idealistic bust.

We will so turn these busts round a vertical axis, that by degrees everyone, in the various parts of the theatre, may obtain a fair front view of them. I wish we were able to turn the revolving table round a horizontal axis as well, in order to avoid the foreshortening, which must be inconvenient to those sitting in the higher rows of the theatre. This, however, would have required too complicated an apparatus.

The idealistic bust is the portrait of a great poet: the realistic that of a natural philosopher. I dare say that at first sight the greater part of the audience, especially those sitting at a distance, will prefer the idealistic bust. We shall see at once whether this feeling will hold good, when we compare the two busts with reference, *firstly*, to technical execution; *secondly*, to anatomical correctness and truth to nature; *thirdly*, to likeness; and, *fourthly*, to life-like expression and intelligent conception of the individuality.

To begin with the technical execution. On superficial inspection, the smooth and soft surface of the idealistic bust may speciously charm the eye; a somewhat closer examination, however, will show that a vagueness of outline and want of detail are concealed behind this easily attainable external finish, and that this particular idealistic bust at least is greatly inferior to this realistic one. In a still higher degree this is the case, when truth to nature and anatomical correctness are considered. If we hold this skull close to the realistic bust, to whatever part of it we may direct our attention, we shall always be able to discern the same correct proportions in the corresponding part of the bust; and, so to speak, to trace the bone in it. In the other bust, on the contrary, anything as well as a skull might be hidden below the conventionally-shaped surface; and in those places where the shape of the bone is but little covered by fleshy parts, as, for instance, at the forehead, the temples, the nose, and the under jaw-bone, it is easy to prove the anatomical impossibilities. The third point, that of likeness, might appear to be entirely implied, since anatomical anomalies necessarily include unlikeness. This, however, is only partially true, for a certain likeness of the whole physiognomy is not incompatible with dissimilarities in some of the features, as is proved by caricature. Two portraits in oil, by Phillips, one of which (the original) is in the possession of Lord Lovelace, and a copy of it in the National Portrait Gallery, and the other in possession of Mr. Murray, as well as more than twenty different engravings, which are to be seen in the British Museum, and which represent Lord Byron at different periods of his life, from boyhood to the time of his death, provide us with a sufficiently exact idea of his personal appearance to enable us to judge of the likeness of this bust. If I deny this likeness, you may, perhaps, feel inclined to reply: "But we recognized it at once." Allow me, however, merely to cover with two fingers the well-known curl over the forehead, and then to ask you whether you would still recognize the original. What remains uncovered, although it is the entire face, might as well belong to any other person—or, to speak more correctly, it could belong neither to Byron, nor to any other specimen of the human race.

On the other hand, if we cover any portion of Dr. Ray's face, the uncovered part will characterize the individuality of that learned man, as well as his own face would have done if partly covered in the same way. And we can easily prove the fidelity of the likeness by comparing the bust with the different engravings of Dr. Ray, one of which, in the British Museum, made about the same time as the bust, resembles it most.

But the most striking difference between the two busts is shown when they are compared as to lifelike expression, and the intelligent conception of individuality.

How shall we imagine a spark of that wild poetical genius flashing out of these flat button hole-shaped eyes without pupils? How can we imagine the sarcastic smile of the merciless satirist playing round these stiff conventionally-shaped lips?

How different is the bust of Dr. Ray! Full of life and truth, it shows the grave, earnest look of the keen observer of nature, the deep lined features of the unwearied toiler, who published most important books in such widely different subjects as botany, zoology, philology, and theology. It is true that we can only fully appreciate the expressiveness of these features when we look at them in the right light; for if we change the light, so that it comes from the front and from below, the whole expression vanishes, or, at least, is entirely changed; and while, under such a light, the idealistic bust becomes almost like a piece of white paper, the realistic bust, you will observe, having much more to lose, appears to still greater disadvantage.

After what I have said, it might appear as if I wished to speak against idealism in portraiture altogether. I wish to guard against being thus misunderstood, and therefore state expressly that it is to one special tendency, falsely called idealistic, and of which this bust is a specimen, that my objections are directed; for I consider the true idealism in portraiture consists in something widely different from the erroneous imitation of mere externals of ideal classical works.

We will now show the busts in light coming from below, and for those who are at too great a distance to be able to distinguish the details of the features, we will show two photographs of each bust, the one taken in light coming from above, at an angle of 45 degrees, the other taken in light coming from below at the same angle.

If I look at these busts in this bad light (and to such light busts are only too frequently exposed) I no longer wonder at the remarks one occasionally hears in conversation, such as, "Did you ever see any likeness in a portrait bust?" or, "I am very fond of painting, but I cannot see anything in sculpture." Such remarks express the feelings of a great part of the public with reference to sculpture.

After what we have seen, the idealistic, stylistic, classic artist, or whatever you choose to call him, will have the advantage over the realistic artist when their respective works are seen in the same bad light, and he might feel himself confirmed in his tendency if he reckons that his busts will never have the chance of being seen in a better light. On the other hand, the realist must take care not to be misled in the opposite direction, by the conviction that the merits of his tendency can only be appreciated in a perfectly correct light; for confiding in the sharpness of that light, he might be induced to render details which are not characteristic and essential, but merely accidental.

Now the question arises, What should be called *accidental* and what should be called *essential* in a face? Each artist will give a different answer to this, and the view he takes will be characteristic of his whole tendency, and, in any given case, will determine the nature of his work. This might seem to imply that nothing generally applicable could be said on the subject. This, however, is not the case, for up to a certain point it is possible to determine what must be considered essential, and not only the artist but even the public may derive some advantage from such an analysis.

Let us start from an anatomical point of view, and begin with the bone. Concerning the skull, the question may be easily and absolutely answered thus: Nothing in the skull ought to be considered accidental and of secondary importance. The general proportions, as well as each detail of the shape, must be respected as characteristic of the individual, and scrupulously adhered to without any arbitrary modification. There is no occasion for supporting this principle by any phrenological or physiognomical consideration; the undeniable fact being that, in a portrait most excellently finished in every other respect, the slightest arbitrary modifications of the skull will never fail to produce a defect in likeness. All painters of good portraits, whatever may be the manner they adhere to, whether they have an idealistic or realistic tendency, will be found to submit strictly to this law.

Not so the sculptors, at least not those belonging to a certain school. Perhaps the forehead of the Olympian Jupiter, the neck of the Apollo Belvidere, the thorax of the torso of Hercules, are before their mind's eye, when, having to make the portrait of a mortal, they indulge in modifications which classical antiquity was never guilty of in the portrait. The consequence of this is not merely that the likeness of the portrait is lost, but, moreover, a production results which the natural philosopher on close examination must declare not to belong to the human species in its present state of development. The custom of honouring civic virtues, liberality, activity for the public good, &c., by transmitting the memory of those who had such merits to posterity by means of portraits and busts, affords us ample opportunity for comparison between the portraits of the same person made by sculptors and painters. We may thereby easily convince ourselves of the falsification of individuality—I can use no milder expression—into which sculptors of the so-called classical or idealistic school in portraiture have an irresistible tendency to fall.

In order to settle the question which of the two was right, the painter or the sculptor, in making two entirely different portraits of the same person, it is not absolutely necessary to know the original, where the features and especially the formation of the cranium and the proportions of the facial bones in the bust contain anatomical impossibilities. Let no one imagine that it would be easy or even possible to alter the shape of the human skull and yet to remain within the limits of physiological truth.

It is a more difficult and complicated task to answer "what is essential and what is accidental" with reference to the skin. Whilst one school of painters renders only a general impression of colour corresponding to the complexion of the individual, the realists copy carefully all the minute irregularities of the skin. Among the sculptors the same contrasts are met with. Some imitate every small detail in the skin, whilst others do as little justice to its natural peculiarities as to any other anatomical element. Thus they work the surface of the marble as smoothly as possible, giving, it is true, by skilful manipulation, a very soft and smooth appearance to the stone, but nothing which reminds us of flesh and blood. The hair, the flesh, the drapery, and the pedestal, all appear to be of the same substance, and remind us only of the material of which the artist forms them, and not of those substances which he intends to represent. The neglect of anatomical details might be traced to the fact, erroneously applied to portraiture, that the Greeks in their *ideal* compositions did not mark the muscles and veins. In the Greek as well as in the Roman portraits, however, we find the character of the skin carefully expressed; and all characteristic lines which are the lasting effect of certain motions of the facial muscles recurring frequently in the habits of the individual are thus carefully rendered. In order to get thoroughly acquainted with these lines and folds, we must study the muscular actions by which they are produced. Let us begin with the forehead.

The muscles of the forehead are very thinly and flatly spread out in a state of repose, therefore they alter but slightly, through their substance, the shape of the forehead as determined by the bone. They are, however, of the greatest importance in various mimic motions of the face, inasmuch as they produce the folds in the skin of the forehead, and also the position and alteration of shape of the eyebrows. The broadest of these muscles, the frontalis, by means of its chief substance lifts the eyebrows straight up, and thereby forms on the forehead parallel folds which, with slight undulations, run horizontally over the whole forehead, bending down to the temples on both sides in curved lines. According to the thickness of the skin, and especially according to the degree of thickness of the layer of fat underlying the skin, these folds are more or less numerous, and broader or narrower; and they are most numerous in lean old men with thin skin. In the treatises on mimics and physiognomy, and on anatomy of expression, these horizontal folds of the frontal skin, and the uplifting of the eyebrows, are usually spoken of as giving to the face the expression of attentiveness, of astonishment, and of cheerfulness. It is a new proof how irrational it is to characterize such motions of the muscles taken isolatedly, because, according to their combination with other movements of other parts of the face, just the contrary physiognomic expression may be produced.

Thus the horizontal folds and the lifted-up eyebrows are certainly expressive of attentiveness and astonishment, if the eyes are widely open; on the contrary, if the upper lids are only imperfectly lifted, those actions of the frontal muscles produce the expression of fatigue and drowsiness. Even without the real existence of drowsiness or fatigue, this expression will be found in all cases in which the muscle which lifts the upper eyelid has become weak, or, what comes to the same thing, where, in consequence of the lengthening of the skin of the eyelid the task of lifting the eyelid has become too heavy for the strength of the muscle whose function it is to lift it. The material effect of such conditions is much stronger than that of the mimic motions, which, even if they have become a habit, nevertheless do not act mechanically in such a decisive manner as those constant contractions which are required to assist the insufficient muscle of the eyelid.

As an instance, I show you here the cast of a marble bust from the fifteenth century, which is in the Museo-Nationale at Florence. It is done by Benedetto di Majano, and is worked in the manner characteristic of the Florentine school of that period. You see that the horizontal folds on the forehead, which are very strongly expressed, and strictly copied from nature, do not at all produce the expression of astonishment, attention, or cheerfulness. They evidently do not indicate any transient expression such as springs up suddenly, and dies away as quickly, and which, for that very reason, would be unfit for a portrait; but they do indicate such an habitual expression as was characteristic of the appearance of the individual. Measurements will prove this very easily. The eyebrows (you will observe) are, at their extremities, lifted up a quarter of an inch above their original length. Though the skin is considerably stretched between the eyebrows and the margin of the upper lids, yet above the cartilage of the lid a broad fold has been formed, and as, at the same time, the slit of the lids is only very moderately opened, it shows clearly that the skin of the upper lid must be so considerably lengthened, that without the effort of the muscle of the forehead, and the lifting of the eyebrows, it would necessarily overhang the lid. Thus, the opening of the eye would overtask the strength of the muscle to which this function pertains. Imagine to yourselves this bust with smooth forehead, and the likeness would at once disappear. Therefore it is that such folds of the skin must not be considered as accidental, but as essential.

It is the same with the vertical folds of the forehead. They are produced by the contraction of two muscles proceeding from the centre of the lower margin of the forehead, and taking a horizontal direction towards the eyebrows. They draw together the two eyebrows, and thus form in the middle of the forehead, just above the upper part of the nose, one or more vertical folds; and if the contraction is very energetic, a series of short vertical folds may be seen also above the inner half of the eyebrows. The mimic expression of seriousness, of effort, of deep meditation, of passionate anger, is transiently thus produced. Any lasting or habitual contraction of these muscles, caused by any reason whatever, leaves stationary vertical lines and folds on the forehead, as you see in this portrait of the Bishop of Fiesole, made by Mino da Fiesole, the original of which is in the Church of Fiesole; and also on this portrait of an unknown of about the same period. Only very rarely we find on the forehead, as a lasting feature, that form of folds produced by the simultaneous straining of all the muscles of the forehead, which gives an expression of the deepest pain. There the eyebrows are drawn together with their inner extremities, and, at the same time, drawn much upwards, as is so beautifully expressed in the Laocoon. But, for the portrait, this folding of the skin of the forehead, and this position of the eyebrows, need scarcely be taken into consideration, because they do not form any lasting physiognomical characteristic.

A lasting fold is formed by the skin, to a degree increasing with age, at the two sides of the mouth, descending from both sides of the nose. On the cheeks and under the chin regular folds, such as appear in the bust by Benedetto di Majano, are only seen in later age, and even then but rarely so strongly marked as in the before-mentioned bust. On the contrary, numerous minute folds will show themselves much earlier in life in those places of the facial skin where it is thinnest, that is to say, above the upper and under the lower eyelid up to the margin of the socket of the eye. Here the slightest modifications in the quantity of fat below the skin manifest themselves so distinctly that alterations may be observed even at different times of the day, and may be very quickly produced by any little change of the general state of health. This kind of formation of folds in that part of the skin exercises great influence on the expression of the face.

The blood-vessels of the face are of importance for the painter in so far as the colour of the skin in the different parts of the face depends upon them. For the sculptor, only certain large veins are of importance, which, especially with men in later life, come out so strongly on the forehead, the temples, and the neck, that they stand out from the surface of the skin, and have a characteristic influence on the countenance. In the body, and chiefly in the extremities, the muscles are visible, even if not contracted. In the face they do not come out separately, firstly, because they are so thin, and secondly, because they are so much covered by a relatively thick layer of fat and by the skin, that only their effect on the form and movement of the covering parts can be noticed. Let us imagine these covering parts removed, and glance briefly on those muscles the exact study of which is of such great importance for the artist who has to represent the human face. The muscles of the forehead we were obliged to describe in order to explain their effect on the formation of folds in the skin of the forehead. On the nose, besides those fascicles of frontal muscles which go down to its upper part, we find two muscles: first, one originating from the nasal bone and descending on both sides towards the nostrils, which, by contraction, opens the nostrils, as in the process of inspiration; secondly, one arising from the upper jaw, and descending partly to the nostrils and partly to the upper lip. This muscle lifts simultaneously the nostrils and the upper lip. Its opponent is a small muscle arising from the upper jaw above the front teeth. It is inserted into the lateral cartilage of the nostril and pulls it down. The contraction of these muscles, with the exception of the two which lift the nostrils and upper lip, usually produces only a very small but almost continual motion, viz. that which accompanies respiration, and which only under the influence of passion will be found to increase. This movement, however, though slightly apparent, is very important for the expression, inasmuch as it keeps the nostrils in that position which is characteristic of life. With the last breath, and with the consequent relaxation of muscles, the nostrils collapse, and this greatly contributes to produce the expression of death.

The common elevator of the nostrils and upper lip, if contracted very strongly, produces the expression of suffering or of disgust; if only slightly contracted, it combines with the other muscles of the cheek in their action upon the lips.

The movements and alterations of form in the lips, so very important for the expression, are produced through the alterations of tension in a circular-shaped muscle, which constitutes the principal substance of the lips. It is called the *orbicularis oris*, and the muscles coming from the upper and lower maxillary bone, and directing themselves towards its margin, act upon it in the most various combinations. These muscles are—

1. The elevator of the nostrils and of the upper lip.
2. The proper elevator of the lip.
3. The elevator of the corner of the mouth.

These three arise from the upper jaw near the orbit, and then joining themselves to the upper lip and the angles of the mouth, lift these parts by their contraction.

4. The zygomaticus arises from a process of the cheek-bone which joins the temporal bone, and is inserted into the angle of the mouth. The angle of the mouth is drawn down by the *depressor anguli oris*, arising from the base of the lower jaw, and is inserted into the angle of the mouth. The lip itself is drawn down by the *quadratus menti*, whilst the *levator menti* alone accomplishes the task of lifting the chin, and, in common with the last-mentioned muscle, enables one to pout the under lip. By strong contraction of these muscles, all the motions of the face are produced which are required for the expression of the most various passions and emotions. The consideration of these passions and emotions lies outside our subject, because any passionate expression must be unfit for representation in a portrait, since from its very nature it cannot be lasting. The small vacillations, however, in the tension of the above-mentioned muscles, are of the greatest influence on the portrait, because they produce those minute modifications of form in the lips and cheeks which, combined with the analogous effect around the eyes, are the principal contributors to the expression of the human face. Their correct choice, on the painter's part, and the true rendering of them, are identical with the right understanding of the individuality, and make the painter as well as the sculptor avoid the danger of giving to the face rigidity, lifelessness, and want of expression, instead of the repose that becomes a portrait. .

The conditions acting on the mouth and on the eyes are much more similar than one might suppose to be the case. To begin with, we must be clear that what is commonly called the expression of the eyes depends only in a very slight degree on the eye itself, that is, on the eyeball, and in the whole remaining degree on the conditions of the surrounding parts. The movements and alterations of form of the eyelids are produced in quite an analogous manner to those of the lips, viz. through the contraction of a circular-shaped muscle, the *orbicularis palpebrarum*, and of its two opponents, viz. 1, a particular elevator-muscle of the upper lid; and 2, the different muscles which displace the frontal skin and the eyebrows, and which we described when speaking of the forehead.

In the same manner as the circular-formed muscle of the mouth, that of the lids is apt to contract itself either as a whole, or only in its single parts, or to contract one part more than the other, according as it has to prevail more or less over its opponents. The mechanical effect which is thus obtained on the position of the margin of the lids, and on the shape of the whole slit of the lids, is certainly neither as extensive nor as manifold as the analogous effect of the much more complicated apparatus of the muscles of the cheeks and lips on the form of the mouth. However, the influence upon the expression is by far greater with the eye, inasmuch as the minutest alteration in the margins of the lids would have very considerable effect upon the aspect of the eyeball.

In order to account for this, let us set apart for the moment all other influences, and supposing all the other parts of the face to be covered, and the eyeball itself perfectly immovable, the pupil looking straightforward, let us study, in one eye, the influence of the smallest alteration in the slit of the eye on the expression. We see then that the upper lid moves up and down like a curtain before the eyeball, not however in a plane, but adapting itself to the surface of the eye. This curtain at the angles of the eye is connected with another curtain formed by the under lid, which moves up and down in a similar manner, but in an opposite direction. These two connected lids cover by far the greater part of the eyeball, so that only a somewhat oval section of it becomes visible. It is this section alone which, from an æsthetic point of view, we are accustomed to consider as the eye, and the size and form of which produce the apparent size and form of the eye. I say apparent, because in reality this latter, of course, undergoes no alteration at all. If we speak of large or small eyes, we only understand by it this apparent size, which depends entirely on the width of the slit, and the conditions of the orbit. The real size of the eye is altered only in certain anomalies, for instance at a very high degree of short-sightedness. This is not meant to imply that the effect on the expression is exclusively produced by the surroundings of the eye, and not at all by the eyeball itself. We may prove this by another experiment, where we leave the lids and eyebrows immovable, while the eyeball is made to turn on its own axis from the right to the left. Of course the pupil also moves inside the slit from one end to the other, altering the expression considerably even in one single eye, much more so if the movement is observed simultaneously in both eyes, and most of all if this movement is not only a lateral one, but is combined with an upward rolling of the eye, which naturally implies an alteration of form in the slit of the eyelid. Those movements which alter the direction of the visual line of each eye, and the relation existing between the visual lines of the two, and which also alter the position of the cornea and pupil in the opening of the eyelid, constitute what we call the look. To the look, painters, in their compositions as well as in their portraits, have always paid great attention. In several of the old masters it struck me that they had a sort of predilection for a certain direction of the look. Thus Murillo gives the preference to a look of extasy directed straight upwards, whilst Guido Reni has a certain predilection for painting an eyeball turned obliquely upwards, and he gives to this direction of the look quite a peculiar character by the posture of the head, and position of the eyelids and eyebrows, imitated from the Laocoon or the Niobe. In his numerous portraits Vandyke evidently prefers the following direction of the gaze. His portraits look somewhat to the side of the observer into the distance, and in such a manner that the heads turned to the right have their eyes directed to the left; and, *vice versâ*, the heads turned towards the left have their eyes directed so far to the right, that sometimes the iris comes quite near the right angle of the slit of the lids. Those of his portraits that have been shown in this year's exhibition of the Old Masters confirm this observation. They are twelve in number, and they have all of them the above-mentioned direction of the eyes. Of course those portraits are excepted which represent two persons speaking to each other; for instance, that of Rubens, by Vandyke, in the National Gallery.

In sculpture, the representation of the look is more difficult. There is, above all (at least in our time), the want of colour, which, by defining the pupil and the iris that surrounds it, clearly indicates the position of the eyeball. Nevertheless the direction of the eyes is to be recognized even in the ideal compositions of classic antiquity by the position of the eyelids and the shape of the visible part of the eyeball. In the portrait, however, especially in the purely iconic representations, the Greeks used to indicate the pupil by a small flattening of the eyeball, by which the shadow of the upper lid became broader, or they indicated the look in the way on which you see it here in the busts of Demosthenes, Pericles, and Alexander the Great, the beautiful originals of which are in the British Museum. In the realistic busts of the Roman time the pupil was indicated by a small hollow which gave shadow, and the margin of the iris or cornea was designated by a fine engraved circular line. The same or a similar manner of indicating the pupil has been since adopted by the majority of sculptors, and it is to be considered as a misunderstanding if sculptors of a certain tendency believed that they imitated the antique in the portrait bust, when they left the eyes perfectly expressionless, without pupil, with the lids and the eyeball shaped in a conventional curve, by which the inanimate look of the rest of the features was considerably increased.

The utility of anatomical and physiological knowledge for the artist has been repeatedly denied. As a chief support for this assertion, it has been alleged that the Greeks did not know anatomy, and moreover that the knowledge of anatomy possessed by the artists of the Renaissance and modern times rather led them to an exaggerated representation of the muscles, and caused them to indulge in complicated and unusually difficult positions of the body. We must, however, remember that though the Greeks certainly did not possess the same facilities for studying anatomy as the artists of present times, they notwithstanding possessed a perfect knowledge of that which was essential for them in anatomy. They knew the skeleton. One, cast in metal, existed in the temple at Delphi, to which Hippocrates had presented it. They possessed descriptions of anatomy; they dissected animals; and they could complete the knowledge thus acquired by the observation and the study of the nude form, not in the state of repose or weariness of a tired model as now, but in the free display of the muscles shown by the boxers and wrestlers and other competitors in the public games.

Without such means of correcting and completing anatomical knowledge, derived only from the study of the dead body, the artist might be led to adapt it incorrectly to life. Of course this does not imply anything against the usefulness of anatomical knowledge; it only proves the necessity of completing it physiologically. As for the portrait, the advantageous influence of a thorough knowledge of anatomy, and of physiognomy based upon it, will be clearly manifest. It will preserve the realist from the petty imitation of mere accidental details by the conscious observation of those physiognomic movements of the features which are characteristic of the expression. He will learn how to ennoble his works by animation and intellectual expression, and thus he will approach the true idealism; for it is in such refinement of the material, through a clear understanding of the individuality, that we see the true idealism in portraiture, and not in mere embellishment of the features as to colour or form.

The study of anatomy is still more important for the artist belonging to the classical school. Unfortunately this view is not shared by many of those who have to regulate the course of studies in schools of art. Thus we see the artistical education often based exclusively on the copying of the antique. The consequences of this system are the imitation of certain externals, without a clear understanding of what is essential in classic art; and the incapacity of observing nature with an open mind and unprejudiced eye. Thus a tendency develops itself among the sculptors which produces those portraits falsely called idealistic, whose want of merit has not yet been sufficiently stigmatized. There is neither beauty nor art, neither truth nor power, nothing but mannerism and specious hollowness; and the pretended classicality only serves as a cloak for indecisiveness and emptiness. In this we see not only danger for the artist and his school, but at the same time a danger for the taste and interest of the public. Therefore, from this spot devoted to natural science, may I be allowed to say: Go back to nature, to the true and conscious observation of nature. They are only mediocre artists who are afraid to find in the clearness of science an impediment to their poetic flights and the inspiration of their genius. The great artists of all times knew of no such hindrances, but based all their works on the most careful study of nature, whatever were the ideals they aimed at; for they all knew that there is no beauty without truth.

## GENERAL MONTHLY MEETING,

Monday, April 5, 1875.

THE DUKE OF NORTHUMBERLAND, D.C.L. President, in the Chair.

Witham Matthew Bywater, Esq.
Charles Elliot, Esq. C.B.
The Lady Caroline Kerrison,
Hugo W. Müller, Esq. Ph.D. F.R.S.
Francis Peacock, Esq.
The Hon. Sir Colley Harman Scotland,
Henry Charles Stephens, Esq.
Capt. Antony Olivier Tabuteau,
Sir Harry Verney, Bart. M.P.
Richard Wheen, jun. Esq.

were *elected* Members of the Royal Institution.

The Managers reported, that they had this day appointed PROFESSOR ALFRED H. GARROD, Esq. Fellow of St. John's College, Cambridge, Fullerian Professor of Physiology, in pursuance of the Deed of Endowment.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, *viz.* :—

FROM

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# Royal Institution of Great Britain.

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## WEEKLY EVENING MEETING,

Friday, April 9, 1875.

William Spottiswoode, Esq. M.A. LL.D. Treasurer, R.S. Secretary and Vice-President, in the Chair.

Sir William Thomson, D.C.L. F.R.S.

PRESIDENT OF THE ROYAL SOCIETY OF EDINBURGH,

### *On Tides.*

Julius Cæsar, speaking of the night following the fourth day after his first landing in Britain, says:—"That night it happened to be full moon, which day was accustomed to give the greatest risings of water in the ocean, though our people did not know it." So, though a storm was raging at the time, nothing was done to secure the transports left high and dry on the beach, while he was fighting the Britons; and the water rose so high in the night as to damage them seriously. Strabo, quoting Posidonius, says that "soon after moonrise the sea begins to swell up and flow over the earth, till the moon reaches mid-heaven. As she descends thence the sea recedes till about moonset, when the water is lowest. It then rises again, as the moon sinks to mid-heaven under the earth, then recedes again until about moonrise, when the water is again at its lowest, and again begins to rise;" and so on. Besides this daily circuit of the sea, Posidonius spoke of a regular monthly course, and of an annual course, reported by the Gaditani; his description being, in fact, a fairly accurate account of what is observed at the present day at Cadiz, the locality whence he derived his information.

In commenting on these statements, the speaker said that the mere mention of these courses indicated that the daily rise and fall of the sea are related to the sun as well as to the moon. Galileo criticized Kepler for assuming that the motions of the water depend upon the moon; but Kepler was proved to be right. According to Newton's theory, the moon's and the sun's attractions are the tide-generating force.

By models and diagrams the phenomena of the tides were illustrated, and it was shown that there are two distinct ways by which the sun, but only one way by which the moon, produces motions of water on the earth—the sun by his heat and attraction, the moon by her attraction only. It was also explained how regular daily and annual variations of the sea-level are produced by the sun's heat, and diurnal and semi-diurnal and annual and semi-annual changes of level produced by the sun's attraction.

In conclusion, after stating that theory is utterly weak to calculate tides beforehand, the speaker exhibited an improved form of self-registering tide-gauge, which shows the height of the tide at every moment for the whole time the instrument is in action, together with a roll of paper illustrating the working of the tide-gauge at Liverpool. He then adverted to the tide-calculating machine, an important instrument invented to utilize the results of the tide-gauge. In preparing the results for this apparatus there is a process of harmonic analysis by which the complicated action derived from the tide-gauge is analyzed into its several constituents, each representing what is called a simple harmonic motion. This harmonic analysis, when it is worked out, becomes, as it were, a piece of scientific book-keeping, and any number of tide-tables may be constructed according to proper summations of averages. This, though an exceedingly simple, is a very laborious process; but by the tide-calculating machine tide-tables may be calculated beforehand for any port in three or four hours of mechanical working. The result shows not merely the times and height of high water, but also the height of the water at every moment of time, day and night, for the year. By the use of this machine the labour of the hydrographic department of the Admiralty would be enormously diminished.

## WEEKLY EVENING MEETING,

Friday, April 16, 1875.

WILLIAM SPOTTISWOODE, Esq. LL.D. Treas. R.S. Secretary and Vice-President, in the Chair.

PROFESSOR GLADSTONE, Ph.D. F.R.S.

### *The Progress of Science in Elementary Schools.*

THE speaker commenced by defining his subject more fully as the "Rise and Progress of Natural Science in the public Elementary Schools of the United Kingdom." He then briefly recounted the history of elementary education, and the origin of the large educational societies. He showed that till recently natural science had scarcely any place in the instruction of the masses, though the efforts of such men as the Rev. Arthur Rigg, for a long time principal of Chester Training College, and of the late Dr. Birkbeck, and Lord Brougham, were worthy of all honour.

The Great Exhibition of 1851 seemed to show to the people of England that they were deficient in the knowledge both of art and of practical science. This was followed very shortly by the establishment of the Science and Art Department. The Great Exhibition in Paris in 1869, gave a second and perhaps still more powerful lesson in the same direction; and many of our statesmen—Dr. Lyon Playfair and others—took up the matter, and showed that unless we did educate our artisans more thoroughly we should fall back in the race, and that our manufactures would fail in the competition with Continental nations. Thus the movement which had been started by Christian philanthropy received an impulse from more selfish considerations; and it was determined by the British people that education should be more general, if not universal; while at the same time there rose a great desire that the education should be of a more scientific character than it had been hitherto.

In speaking of the introduction of science into our schools we have to consider three different classes of persons. There are the schoolmasters and mistresses, who must have some knowledge of science before they can teach it; the pupil-teachers; and the scholars.

With regard to the teachers, the training colleges established in connection with all the large educational societies have not been slow in taking advantage of the offers of the Government, and forming science classes. In this way we find that the large bulk—almost all—of these young men and young women who are in training to be schoolmasters and schoolmistresses attend certain classes in science now, go up for the examination of the Science and Art Department, and thus take their elementary or advanced certificate in science. In fact, last year there came from twenty-eight of the training colleges of England and Scotland alone—according to the Directory—2343 students in science. The following Table indicates the sciences which they took up.

| Subject. | Training Colleges (1874). | | St. Thomas Charterhouse S.S. Students (1875). |
|---|---|---|---|
| | Colleges. | Students. | |
| Theoretical Mechanics .. .. .. .. .. | 9 | 424 | 40 |
| Acoustics, Light, and Heat .. .. .. | 6 | 344 | 136 |
| Magnetism and Electricity .. .. .. | 9 | 456 | 120 |
| Inorganic Chemistry .. .. .. .. .. | 4 | 314 | 75 |
| Geology .. .. .. .. .. .. .. .. .. .. | 7 | 316 | 12 |
| Mineralogy .. .. .. .. .. .. .. .. .. | 1 | 50 | .. |
| Animal Physiology .. .. .. .. .. .. | 19 | 1097 | 197 |
| Elementary Botany .. .. .. .. .. .. | 4 | 201 | 70 |
| General Biology .. .. .. .. .. .. | 2 | 165 | 35 |
| Physical Geography .. .. .. .. .. | 25 | 1539 | 230 |

The mathematical sciences have not been included in the above table, nor those that are distinctly allied to technology. It will be seen that the largest number of students attended classes of physical geography. Next to this subject comes animal physiology. Almost all the leading colleges take up this. There are two, however, both colleges for young women — that at Southlands, belonging to the Wesleyans, and that at Liverpool, belonging to the Roman Catholics —which instead of animal physiology take up the somewhat allied subject of general biology. Magnetism and electricity seem to be the next favourites, especially with the National School Society, and in Scotland; and then acoustics, light, and heat. The study of chemistry is not so general, being taken up by only four of the colleges. They happen, however, to be among the most important. There exist also good laboratories; for instance, at the training colleges in Westminster and the Borough Road.

The speaker then drew attention to the next column—the statistics of the St. Thomas Charterhouse School of Science in the Goswell Road. This was established entirely for teachers—not necessarily adult, for there are many pupil-teachers. The present is the third season of its existence. It was started by the Rev. John Rodgers, the vicar of St. Thomas Charterhouse, and is carried on mainly under the direction of the National schoolmaster, Mr. C. Smith. There were 200 students the first year, 450 the second, and this year there have been 550 students. Physical geography is the greatest favourite, as in other places; then animal physiology.

The Science and Art Department have arranged that certain teachers—acting teachers—should attend practical classes in the summer time at the School of Science, South Kensington. The classes last for three weeks. The teachers are chosen from those who apply, and have this instruction gratuitously afforded to them, and every expense paid in coming up to London. A great number of these students come over from Ireland. They study mechanics, botany, chemistry, physics, mathematics, and so on. There were 170 last summer. The speaker saw forty or fifty of these men, all practical teachers, in Professor Guthrie's laboratory. They had been listening to a lecture that he had given, and Professor Barrett was instructing them in the making of apparatus. These students were all men educated to a certain extent in the subject which they were considering; but they had not previously seen the apparatus, or experiments. They soon learn how to make various things; and through the kindness of Professor Guthrie the speaker was able to put upon the table a large collection of pieces of apparatus made by these schoolmasters last year, including an air thermometer, a dissected Leyden jar, an electroscope and discharger, a Wheatstone bridge, a monochord, a spectroscope, a thermopile, a barometer, and many other physical instruments, plain but perfectly efficient. Professor Frankland gives instruction in practical chemistry to a similar class of acting teachers. They conduct simple analysis in his laboratory at South Kensington.

Of course these various classes give a considerable amount of scientific knowledge to the teachers who attend them, and in the London School Board every encouragement is given to the schoolmasters and schoolmistresses to obtain certificates from the Science and Art Department. Preference is given to those that have such certificates. The salary is 5*l.* a year more for every male teacher who brings with him three advanced science certificates; and if a female teacher brings one advanced certificate she has 2*l.* 10*s.* a year additional. There is a somewhat similar arrangement made in the Irish Model Schools, where 10*l.* extra are given to a master or mistress who actually teaches physical science in the school.

As to pupil-teachers, science is not an essential part of the Government requirements for them; but additional marks are given if they take up one science subject at the end of their period of apprenticeship before entering the training colleges. This science subject may be mechanics, chemistry, animal physiology, acoustics, magnetism, physical geography, or botany. The Scotch code is a little more liberal, giving marks for two of these subjects. The present proposal of the School Management Committee of the London Board is that male pupil-teachers shall all learn animal physiology and chemistry.* Many pupil-teachers attend the ordinary science classes.

Science is not demanded of the scholars of elementary schools by the Education Department, excepting perhaps a little of what may be called physical geography in the second standard; but there are certain

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* Since this discourse was delivered the Board has placed these two subjects in its "honours course" for pupil-teachers.

"specific subjects," and in the new code which has just become law these comprise mechanics, animal physiology, physical geography, botany, and domestic economy for girls, which last includes cookery. There is an admirable note appended to the table of "specific subjects," which affirms a principle of wide application. It runs as follows:

"It is intended that the instruction of the scholars in the science subjects in this table shall be given mainly by experiment and illustration, and in the case of physical geography by observation of the phenomena presented in their own neighbourhood. If these subjects are taught to children by definition and verbal description, instead of by making them exercise their own powers of observation, they will be worthless as means of education."

Not merely is it allowed that these specific subjects shall form part of the ordinary teaching in an elementary school, but under certain circumstances a Government grant of four shillings a head is to be given for every pupil who passes a satisfactory examination in not more than two of these subjects, so that there will be a pecuniary advantage accruing to the school from the teaching of these various subjects. We shall see gradually how this works; it will to a certain extent stop the learning of science by some of the younger scholars that take up the matter now; yet probably till a boy is twelve or thirteen years old it is not very advisable that he should go in for these examinations. It is astonishing what young children sometimes pass at the present time. The following Table drawn up by Major Donelly will exhibit this.

| Age of Students. | Results of the Examinations of 1873 and 1874 combined. | | | |
|---|---|---|---|---|
| | Elementary Stage. | | Advanced Stage. | |
| | Number who came up in 1873 and 1874. | Percentage who passed. | Number who came up in 1873 and 1874. | Percentage who passed. |
| In their 11th year | 2,110 | 33 | .. | .. |
| " 12th " | 5,283 | 39 | 66 | 33 |
| " 13th " | 9,850 | 44 | 166 | 52 |
| " 14th " | 11,972 | 51 | 470 | 50 |
| " 15th " | 11,148 | 56 | 793 | 52 |

A large number of the above no doubt are pupil-teachers, and many others are scholars in elementary schools. There are certain scholarships offered by the Government under the name of Elementary school scholarships; but few take advantage of them, only thirty-two apparently last year, of whom seventeen were in Scotland.

The way in which science is taught in schools is by means of properly graduated object lessons. The regulations of the School Board for London lay it down as one of the essential subjects to be taught in all the schools: "In the infant schools object lessons of a simple character; and in junior and senior schools systematized object lessons, embracing in the six school years a course of elementary instruction in physical science, and serving as an introduction to the science examinations which are conducted by the Science and Art Department." Liverpool and some other towns have followed London in this respect. Object lessons also have long been given in the best voluntary schools, and boxes containing small specimens of a large number of substances may be purchased, and many excellent diagrams of natural objects have been published. A collection of these, illustrating nearly all branches of science, was exhibited.

The speaker then described an object lesson on a cocoanut, given to a class of children of about six years of age, and showed that the value of these object lessons depends very much upon the amount of knowledge possessed by the teacher, and the study which has been given beforehand to the preparation of the lesson. Then taking a bottle of sand out of the object box, he showed that even so unpromising a material as sand was suggestive of most interesting topics of instruction at least for elder children. He afterwards described a lesson on a snail and the mollusca in general, which he had heard given to girls of about twelve years of age, from one of the natural history diagrams; and he followed this up with a description of lessons on magnetism given to the elder boys in the practising schools of the training colleges at Battersea and the Borough Road, and how the boys were encouraged to make little mariners' compasses for themselves.

The speaker then, while freely admitting the greater importance of moral teaching, contended for the young people becoming acquainted with some branches of natural science. This knowledge will introduce the boys to technical studies for the various handicrafts which they are to pursue in after life, and prepare the girls for their domestic duties. And beyond this, the study of natural history and natural science has a very peculiar and special power in training the judgment in the pursuit of truth. With reference to the question, which is the most important of these science subjects, the speaker expressed a favourable opinion of physical geography, including the knowledge of the world in which we are, its relation to other worlds, and the materials of which it is made; but he spoke more strongly in favour of animal—that is human physiology, especially for girls, pointing out its great importance in relation to health. That the girls in elementary schools will grow up to be mothers themselves, or servants entrusted with the care of children, is a powerful additional argument in its favour. Then he would have some chemical or physical study—something that tells of the various natural forces, and how they act and react. It is very desirable to teach by observation and experiment. Children are always inquiring. To us books are beautiful things: we use them as we use telescopes or microscopes to assist our vision. But a book is a very different thing to those who are not able to read with ease. A book is only a semi-transparent medium to most children. Books of course will be in the hands of the teachers; but let the children see the things themselves first, and become acquainted with them, and then the books will become luminous with intelligence; and they will care to read them, and thus advance in their studies. It is well that the children should have collections of objects, and be encouraged to make little collections for themselves. Cabinets of specimens and models do exist in some English and Continental schools, and the Museum of the Home and Colonial Training College is worthy of imitation elsewhere.

In conclusion, the speaker claimed for natural science an honoured place in the curriculum, being sure that we shall benefit the rising race greatly if we make them well acquainted with the various objects of nature round about them, and the various forces that act upon them. In this way they will have much greater power in the world, and much more chance of success in the great battle of life which they are all to enter upon, for they will be fighting not with unknown but with known combatants; and their knowledge will tend to maintain healthy bodies, and that will not interfere in any way with sound minds or clear consciences.

[J. H. G.]

## WEEKLY EVENING MEETING,

Friday, April 23, 1875.

George Busk, Esq. F.R.S. Treasurer and Vice-President,
in the Chair.

Professor A. C. Ramsay, LL.D. F.R.S.
DIRECTOR-GENERAL OF THE GEOLOGICAL SURVEY OF THE UNITED KINGDOM,

## *The Pre-Miocene Alps, and their subsequent Waste and Degradation.*[*]

The speaker began by referring to his discourse last year,[†] when he expressed his opinion that in pre-miocene times the Alps were as high as they are now, notwithstanding that since miocene times they have been upheaved about 5500 feet. Sir Charles Lyell having expressed his surprise at this statement, and his wish for an explanation of the grounds upon which it was based, led to the working out of the subject in detail, and the preparation of the present discourse. The Professor then alluded to the origin of mountain chains through the contraction of the earth's crust; some parts of this crust sinking, other parts being crumpled and relatively raised. Thus, at a certain period in geological history, preceding the formation of the miocene rocks, which belong to the middle tertiary or cainozoic strata, a disturbance of the earth's crust took place, due to shrinkage, so that the Alpine strata were thrown into highly contorted forms, resulting in a great pre-miocene mountain range, the base of which was just about sea-level. That these pre-miocene Alps did really exist as a great mountain range the Professor proceeded to demonstrate, and by a series of calculations showed the immense amount of miocene deposits formed partly in the sea, but chiefly in lakes only a little above the sea level, such deposits being the proceeds of the waste and degradation of these pre-miocene mountains by various physical causes, similar to those now acting on the earth's surface, and occupying an immense period of time.

In support of his argument, the Professor estimated the thicknesses of the various subdivisions of the miocene strata, and striking an average for the country between Geneva and Constance (about 8000 square miles), he estimated the thickness of the miocene deposits at 4000 feet, containing about 33,037,760,560,000 cubic yards, equal to an oblong rectangular mass, 109 miles long, 18 miles broad, and 15,840 feet high, of solid rocks; a mass of matter equal to that which would occupy an area four times the length and of equal height to Mont Blanc as it now stands, between Martigny on the north-east, the sources of Bonnant on the south-west, the valley of Chamouni on the north-west, and the Val de Ferret and the Allée Blanche on the south-east.

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[*] The lecture was illustrated by diagrams, without which it is difficult to make all the points plain.

[†] See p. 279 of Royal Institution Proceedings, vol. vii.

This was followed by an account of the methods by which he endeavoured approximately to estimate the cubic contents, as they now exist, of the *pre-miocene* strata of the Oberland, from the Rhone between the Lake of Geneva and Martigny, and the country somewhat east of the Upper Rhine, south of the Lake of Constance; and it was stated that the original bulk of the miocene strata, derived from the waste of these pre-miocene Alps, was far more than equal to the cubic contents of the whole of the uppermost 5000 vertical feet of this range.

The last great disturbance of the Alps heaved up the miocene strata which had been previously deposited and more or less consolidated at their base, and by this latter upheaval the range may have been elevated somewhat more than 5000 feet. The question therefore is this: Was the amount of waste and degradation that took place during miocene times, and which resulted in the deposition of the miocene strata at the base of the Alps, equal or more than equal to the post-miocene elevation of 5500 feet? It was; and even if the Alps had suffered no waste since then, they may have been as high before miocene times as they are now. But they have suffered much further waste, by atmospheric denudations, during the later tertiary epochs, during which the various formations of the crag and forest beds were deposited, comprising a series of geological episodes so long, that palæontologically considered they may perhaps be comparable to the Miocene epoch itself. Later still, we must take into account the prodigious amount of degradation suffered by the Alps during the glacial epoch, when on the south the mighty glaciers filled every southern valley, and spread in vast fan-shaped masses far out on the area now occupied by the plain of Piedmont and Lombardy, while on the north of the chain glaciers more gigantic still overspread all the lowlands of what is now Switzerland, till they abutted on the Jura; and all across the space where the Lake of Constance now lies, the old Rhine glacier spread far northward into Bavaria.

To estimate the amount of work performed by such glaciers, both south and north of the chain, we may take as an example the great moraine of the Dora Baltea, which beyond the mouth of the Val d'Aosta forms a vast amphitheatre sixty miles in circuit, about seven miles wide, and in great part 1650 feet in height, and which, in spite of age and decay, still retains an amount of matter that, if piled in mountain form, would be as long as, and have one quarter the height of Mont Blanc. Multiply this by the old moraine matter of the five great glaciers that descended from the south side of the Alps to the valley of the Po, as far east as the Lago d'Iseo; add to that the degrading and transporting power of the five great northern glaciers that overflowed the plains of Switzerland between Geneva and the Lake of Constance; think of all that has been done in the same direction since the close of the glacial epoch; and to all these add the vast amount of matter won from the more ancient Alps to form the miocene strata, as previously estimated; and it seems impossible to resist the conclusion that in pre-miocene times the Alps must have been not only as high, but higher than now, even though since that time they may have been heaved up 5500 feet. The speaker concluded by expressing his opinion that the history of many of the other great mountain ranges resembles that of the Alps, for in a geological sense their epochs of upheaval are the same.

## WEEKLY EVENING MEETING,

Friday, April 30, 1875.

Warren De la Rue, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

Walter Noel Hartley, F.C.S.
DEMONSTRATOR OF CHEMISTRY, KING'S COLLEGE, LONDON,

## *On the Action of Heat on Coloured Liquids.*

All substances whatever may be divided into two classes, the coloured and the colourless. By simple inspection it is impossible to distinguish one colourless liquid from another, but the case is very different with coloured substances. In dealing with compound substances derived from the metals, we find generally that a certain colour is characteristic of a certain metal. Thus a blue is the tint which prevails in most compounds containing copper, while green is characteristic of nickel, and pink or red of cobalt. If we examine the colour of a metallic solution by transmitting light through it, and analyzing this light with a prism, as Dr. Gladstone did in the year 1857,* we then get a spectrum which serves in some degree to identify the metal present in the solution. Taking, for example, three liquids containing the same metal—a green, a purple, and a red solution—the green chloride, the purple sulphate, and the red oxalate of chromium, we have such a diversity of behaviour with chemical reagents, and such a variety of tints, that it is not an easy matter to recognize the presence of the same base unless the light transmitted by those solutions is analyzed by the prism, and if this be done in a wedge-shaped cell, we have the advantage of seeing through many different thicknesses of liquid at one glance. We then get a spectrum in each case with chromium compounds, which has a great similarity, its chief characteristic being that the red, green, and blue rays are transmitted, while all the yellow are cut out. It has long been known that certain metallic solutions darken on exposure to heat, and in this way prisoners sometimes effect a secret correspondence outside the prison walls. The method consists in writing with an invisible solution between the lines of their letters, which is afterwards developed by those who are in the secret. Such solutions have been called sympathetic inks, and one in particular, called Hellot's sympathetic ink, is the chloride of cobalt, which being of a pale pink colour, in the state of a dilute solution, on drying and heating turns a very dark blue. Writing executed with this ink is all but invisible until a hot iron be passed over it, when it appears beautifully distinct, and on cooling disappears again. The Fenian criminal, Barrett, when confined in the House of Detention, was found to have in his possession a small tube containing chloride of gold: in the woollen stockings sent to him was noticed some white powder, which could be shaken out. This was evidently for the purpose of secret correspondence. The powder was found to be sulphate of iron; writing with a dilute solution of chloride of gold would be invisible until washed over with sulphate of iron, which solution developes the characters. The blue colour of cobalt chloride (Hellot's ink) is more easily produced if the salt be mixed with calcium chloride; such a mixture is that used for the little instrument called the chameleon barometer. This, which is in reality a hygrometer, or an indicator of the amount of moisture in the air, consists simply of a piece of paper soaked in the two solutions, protected by a glass and frame. The way in which it is differently affected by dry and moist air may be easily seen. Placing two of these under different bell-jars, one containing a little vessel of oil of vitriol, which dries the air, the other a little water, which of course keeps it moist, the paper in the former will be blue, and that in the latter red; then, if their places be changed their colours change. That a solution of cobalt chloride could be turned blue by the addition of hydrochloric acid, of sulphuric acid, of alcohol, or of chloride of calcium, has for some time been known; it has also been noticed that an acid solution of chloride of cobalt, red at an ordinary temperature, becomes blue if heated, such being also the case with a dilute solution containing alcohol; furthermore, that a dilute solution, if heated under pressure in sealed tubes to a higher temperature than that of boiling water, becomes blue.

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* 'Proceedings of the Royal Institution,' vol. ii. p. 336.

In 1871 I found that a strong perfectly neutral aqueous solution of the purest cobalt chloride, which had been prepared from Claudet's salt, changed colour with the greatest possible ease upon heating. A small tube-full held in the hand changed from deep crimson to a decided purple tint in a few minutes; a little of the solution smeared over the fingers soon became blue; and a quantity of the liquid heated in an open dish became blue at 70° C. It was noticed that a very small tube of flattened glass, containing a solution of cobalt chloride, which was perfectly transparent when cold, upon heating to 70° C. became quite opaque. It was evident that the change from red to blue noticed was not simply a difference in refrangibility of the transmitted light, but an increase in the quantity of light absorbed by the solution. Hence it appeared to be a matter of great interest to examine the change of colour by means of the spectroscope, and even to go beyond this, and examine the action of heat on all coloured liquids by the same means, but especially was it expected that the bromide and iodide of cobalt would yield intensified effects of the same kind. Observations have been made on about sixty different solutions, most of which were prepared from metallic salts. Many of these compounds had been but imperfectly examined, while others were quite new. The solutions were generally made by allowing cold water to stand for some days with the crystallized salt, giving it frequent agitation.

The examinations were made in wedge cells cut out of blocks of glass, and polished; one side of the cell was made by fixing on a piece of plate-glass by means of a screw clump. Two sizes of these hollow wedges were used to facilitate the examination of liquids of different intensities of colour. They were each of the same height, namely, three-quarters of an inch, the larger cells being at their thickest part three-quarters of an inch, diminishing to nothing; the smaller being not more than three-sixteenths of an inch thick. The refraction caused by the thick wedge of liquid was corrected by the wedge of glass which formed one side of the cell. A little copper hot-air cupboard, with two opposite sides of easily removable plates of glass, was used for heating the wedges filled with liquid. These wedges were covered with plates of glass, as far as possible to prevent evaporation. Sunlight was used when possible, and in many cases when this was impossible an argand gas burner was used, of such a construction that oxygen gas could be blown into the flame to increase the brilliancy and whiteness of the light.

The spectra of metallic solutions were noticed as being principally of three kinds: those caused by the uninterrupted transmission of certain rays, such as the blue by salts of copper and the green by nickel; those consisting of continuous spectra interrupted by sharp black bands, such as didymium and uranium salts yield; and those resulting from two groups of rays of different refrangibilities, as the green and red seen in chromium salts, and the blue and red in cobalt compounds. Such spectra are those of dichroic solutions. The difference between monochroic and dichroic solutions is easily shown in the following manner, thus: when the spectrum of the electric lamp is thrown on to the screen, and a cell containing sulphate of copper or nickel is interposed, a simple green band of light appears; if, instead of a spectroscope slit, a round disk is used, only one image of this is seen; and if the image of the carbon points of the electric lamp be thrown on to the screen, when a prism is interposed containing nickel solution, one image, and that a green one, is formed. Taking a dichroic liquid such as chrome alum, we get a spectrum consisting of red and green rays, separated by a band of darkness; a disk of light becomes resolved into two, one of a rich red and the other of a green tint, both colours of great beauty; where the two disks overlap the original colour of the liquid is formed.

If we project the image of the carbon points through a hollow prism of the liquid, two images are formed, one red, the other green. Precisely the same thing occurs with cobalt chloride, but the two colours are purple and orange.

The amount of absorption of light taking place in different thicknesses of a solution is easily observed when wedge cells are used, and it is thus easy to ascertain what change will take place on diluting the liquid, provided water is without chemical action. Liquids presenting a wedge-shaped spectrum alter in colour on dilution, in the ratio indicated by the relation of the angle of the wedge-shaped spectrum to that of the wedge cell. These solutions, having spectra bounded by perpendicular straight lines, are only slightly affected by dilution. Some crystallized chromate of potash was dissolved in as little water as possible, forming a beautiful canary yellow colour. This, when diluted to 12,000 times the original weight of the salt, was not greatly altered, the change being not in depth of tint but in brilliancy. Permanganate of potash shows five black lines in the green portion of the spectrum when much diluted with water, and three of these are perfectly well seen when looking through half an inch of liquid which contains only one part of the salt in 118,000 parts of water. The loss of colour caused by the dilution of ammonio-chloride of palladium is indicated by its wedge-shaped spectrum, and was illustrated by dissolving one grain weight of the salt in water and diluting the dark brown liquid till the colour disappears; a pint and a half of water was sufficient for the purpose. On repeating the experiment with permanganate of potash the liquid remained of a beautiful pink colour, even after five gallons of water had been added.

It is a remarkable fact, that although the first step in practical chemistry is the solution of a substance in water, it has hitherto not been ascertained what really takes place when this operation is performed, consequently we are in ignorance of the chemical constitution of the resulting liquid. The chief reason for undertaking this investigation was the hope that this question, to which very doubtful and unsatisfactory answers could alone be given, might be finally decided, and the work recorded has been well bestowed on the subject.

Most metallic salts form combinations with water; the ordinary carbonate of soda (washing soda) is the carbonate of the metal united with ten molecules of water, which may be separated in the form of steam by heating to 100° C. It therefore contains not far from two-thirds of its weight of water, and many other salts combine with water in the same way. Carbonate of soda, however, is colourless; but many *coloured* substances combine with different proportions of water to form compounds varying in colour; thus we see in the accompanying tabular statement the variation between the colour of the anhydrous salts and their different combinations with water are very striking. Many compounds do not lose all their combined water at 100° C., and these salts are amongst the number.

It is a remarkable fact the substances named in the following table are those which change colour most notably on heating their solutions, hence a number of conclusions have been arrived at as to the internal or molecular structure of these solutions. The spectroscope itself would be quite unavailable in giving us the desired information; but chemical

SUBSTANCES VARYING IN COLOUR,

| Anhydrous. | | Compounds produced at 100° C. from ordinary Crystals. | |
|---|---|---|---|
| $CuCl_2$ | Yellow | $CuCl_2 \cdot H_2O$ | |
| $CuBr_2$ | Black and lustrous | $CuBr_2 \cdot H_2O$ | Dark brown |
| $CoCl_2$ | Lavender; blue when hot | $CoCl_2 \cdot 2H_2O$ | Purple; blue when hot |
| $CoBr_2$ | Vivid green | $CoBr_2 \cdot 2H_2O$ | Purple; blue when hot |
| $CoI_2$ | Lustrous intense black | $CoI_2 \cdot 2H_2O$ | Moss-green |
| $NiBr_2$ | Yellow | ($NiBr_2 \cdot H_2O$?) | Dark red |
| $NiI_2$ | Lustrous intense black | ($NiI_2 \cdot 2H_2O$?) | Dark brown |

research, aided by the observation of optical properties, yields what neither alone could do.

A distinct statement regarding this action of heat here follows:

### *The effect of Heat on Absorption-spectra.*

When saturated solutions of coloured salts are heated to 100° C., 1st, there are few cases in which no change is noticed. 2ndly, generally the amount of light transmitted is diminished to a small extent by some of the more refrangible, the less refrangible, or both kinds of rays being obstructed. 3rdly, there is frequently a complete difference in the nature of the transmitted light. Anhydrous salts not decomposed, hydrated compounds not dehydrated at 100° C., and salts which do not change colour on dehydration, give little or no alteration in their spectra when heated.

Solutions of hydrated salts, and most notably those of haloid compounds, do change; and the alteration is, if not identical with, similar to that produced by dehydration and the action of dehydrating liquids, such as alcohol, acids, and glycerine, on the salts in crystals or solution.

A particularly interesting instance of the action of heat on an aqueous solution is that of cobalt chloride, which gives a different series of dark bands in the red part of the spectrum at different temperatures, ranging between 23° C. and 73° C. Band after band of shadow intercepts the red rays as the temperature rises, till finally nothing but the blue are transmitted. Drawings of six different spectra of this remarkable nature have been made. The changes are most marked between 33° and 53°, when the temperature may be told almost to a degree by noting the appearance of the spectrum. Though to the unaided eye cobalt bromide appears to undergo the same change, yet, as seen with the spectroscope, it is not of so curious a character, the bands being not so numerous.

With cobalt iodide a band of red and green rays is transmitted at

WITH THEIR STATE OF HYDRATION.

| Ordinary Crystals. | | Colour of Solution. | |
|---|---|---|---|
| | | Strong. | Dilute. |
| $CuCl_2 \cdot 2H_2O$ | Blue .. .. .. .. | Grass green .. .. | Blue. |
| $CuBr_2 \cdot 5H_2O$ | Golden green .. .. | Red brown .. .. | Blue. |
| $CoCl_2 \cdot 6H_2O$ | Cherry red .. .. | Deep red .. .. .. | Pink. |
| $CoBr_2 \cdot 6H_2O$ | Deep crimson .. .. | Deep crimson .. .. | Pink, |
| $CoI_2 \cdot 6H_2O$ | Dusky red-brown .. | Dark brown .. .. | Pink. |
| $NiBr_2 \cdot 3H_2O$ | Green .. .. .. | Madder brown .. | Apple green. |
| $NiI_2 \cdot 6H_2O$ | Bluish green .. .. | Yellowish brown .. | Apple green. |

low temperatures; the band of light moves towards the opposite end of the spectrum, with rise of temperature, until it is transferred to such a position that it consists of green rays only. In this instance the change to the eye is more striking when seen without the spectroscope, because the mixtures of red, yellow, and green rays, which are formed during the transition, give rise to very beautiful shades of brown and olive green. Thus a saturated solution at 16° C. was of a brown colour, at − 10° C. it became of a fiery red and crystals separated, at + 10° reddish brown, at 20° the same, at 35° vandyke brown, at 45° a cold brown tint with a tinge of yellowish green, at 55° a decidedly yellowish green in thin layers and yellow brown in thick, at 65° greenish brown, thin layers green, and at 75° olive green. This was shown by heating a little of the liquid in a globular flask while it was rapidly rotated by the hand, the liquid thus being spread in a thin layer over a large surface. An examination of this cobalt salt has shown that there are two distinct crystalline hydrates—the one, formed at high temperatures, has the formula $CoCI_2 2H_2O$, and is of a dark green colour; the other, which contains a much larger proportion of crystalline water, $CoCI_2, 6H_2O$, is produced at a low temperature, and its colour is generally brown, in cold weather inclining to red.

The formation of the dihydrate and the anhydrous compound was beautifully shown in the following manner: A glass plate, upon which was smeared a thin but even layer of the cobalt iodide, was held in the rays of the electric light projected on to a screen. At first nothing was to be seen, but on warming the plate a spot of greenish yellow light appeared, and this spreading in every direction showed itself to be a mass of green crystals; the application of a little more heat soon converted these into the black anhydrous compound.

In the case of didymium nitrate a new line was seen to come into existence. The instances of the third kind were frequently so altered by heat, that the less refrangible group of rays was extinguished partially, if not entirely. Cobalt and chromium compounds furnish examples of this.

The formation of different hydrates of the same salt, each of which has its characteristic colour, is very strikingly shown with bromide and iodide of cobalt; thus a sketch of sea and sky is made with the former, the liquid is pink, and not strong in colour, so that very little is seen until the paper be warmed, when the most beautiful azure tints appear. If carefully managed, the green anhydrous compound may be produced, and so give a greenish tinge to the sea. The red hexahydrate of cobalt bromide is first converted into the blue dihydrate, and then into the green anhydrous compound. But an infinitely more astonishing change is that produced by warming a sketch of foliage and water made with the iodide and bromide of cobalt. It has the appearance of a warm-tinted sepia drawing, which gives the foliage quite an autumnal appearance. On warming, the blue of the sky and water, together with exquisite green tints in the foliage, appear. By varying the quantity of the cobalt iodide put on with the brush, colours varying between the most delicate green of spring-time, the full richness of summer, and all kinds of greenish brown autumnal tints, even black, may be produced. The atmospheric moisture re-forms the original hexahydrated salts, and the colours disappear when the paper gets cold again. Two sketches of this kind were exhibited which resumed their original brown colour in the course of half an hour.

It has been suggested, even quite lately, that when a salt such as a chloride is dissolved in water, it is resolved into a hydrochlorate of a metallic oxide, and that hydrated chlorides are always compounds of this kind. Hence, when cobalt chloride is dissolved in water, change from pink to blue might be owing to the alteration in the dissolved compound, from a hydrochlorate of cobalt oxide to a cobalt chloride.

The first step to be taken is the refutation of this. When a solution of cobalt chloride, bromide, or iodide is dissolved in water, and cold alkali, such as potash or soda added, the precipitate formed is not, as one would expect, a hydrate or an oxide, but an oxy-chloride, oxy-bromide, or oxy-iodide, showing that in solution the halogen is still combined with the metal, and is only partially removed by the more powerful base. Similar basic compounds are formed by precipitating other cobalt solutions, such as nitrate.

Equations I. and II. illustrate the supposed and possible decomposing action of water on cobalt chloride, while III. and IV. show the reactions with alkali which would consequently take place; but as these latter do not represent the truth, it necessarily follows that the former are incorrect.

I. $CoCl_2 + H_2O = CoO \cdot 2HCl$.
II. $CoCl_2 + H_2O = CoO + 2HCl$.
III. $CoO \cdot 2HCl + 2KHO = CoO \cdot H_2O + 2KCl + H_2O$.
IV. $CoO + 2HCl + 2KHO = CoO + 2KCl + 2H_2O$.

I. Hence we may conclude that when a salt is dissolved in water it is not decomposed into a mixture of an oxide and an acid, nor does a compound of the oxide with the acid result.

II. When a salt is dissolved in water it does not necessarily attain its maximum state of hydration.

This was long ago suspected by Dr. Gladstone, who, experimenting with cupric chloride, found that a saturated solution of a deep green colour became blue on the addition of water, and this blue became green again upon heating. Furthermore, the blue liquid could be turned to green by the addition of strong hydrochloric acid. These facts of course have been corroborated by spectroscopic observations, and I have succeeded in obtaining others in support of this. Thus the following saturated solutions change colour when diluted with from four to seven times their bulk of water:

| Saturated Solution. | Colour. | Dilute Solution—Colour. |
|---|---|---|
| Cupric chloride .. | Grass green .. .. | Blue. |
| „ bromide .. | Deep red-brown .. | Yellowish green to azure blue. |
| Nickel „ .. | Dark madder brown | „ apple green. |
| „ iodide .. .. | Yellowish brown .. | „ „ |

As illustrations two cylinders were taken, one containing cupric chloride, the other cupric bromide; the former grass green, and the latter deep brown. The colours were shown by swilling the liquid round the sides of the vessels; on dilution with about a quart of water clear azure blue liquids were produced.

A still more striking example is that of the cobalt iodide solution, which is formed by allowing the dihydrate, or the anhydrous salt to deliquesce; it is of a deep green, and on dilution to a very small extent becomes red.

Regnauld and also Person have shown that on diluting a saturated solution of a salt, as a rule there is an absorption of heat. The latter calls this the latent heat of dilution. The former noticed one or two cases in which heat was evolved on dilution. There is every likelihood that this phenomenon is due to the formation of a liquid hydrate. It is impossible of belief that accompanying such a circumstance there should be no measurable development of heat; and these experiments have proved that in all cases when the change of colour consequent on dilution is great, the heat disengaged is very considerable. On diluting a solution of cobalt iodide till the red colour appears, the thermal effect not only registers several degrees on an ordinary thermometer, but it may be perceived by the hand.

The author has not yet completed any exact thermal measurements bearing on this matter, though some experiments have been made in this direction.

III. When a hydrated salt is dissolved in water to form a saturated solution at the ordinary temperature its crystalline molecule remains chemically intact, except in certain exceptional cases, when it appears to lose water.

The evidence bearing on this consists of the fact that most solutions of hydrated salt, if saturated, are of the same colour as the solid compound; though in the state of dilute solutions on the one hand, or of partial dehydration on the other, the colour may differ widely from this. In support of this we have the case of cobalt chloride and cobalt iodide, more especially the formation and behaviour of the green solution of the dihydrate of the latter salt. The molecule of the salt is neither hydrated nor dehydrated on solution, otherwise such salts as those named would change colour.

An exceptional phenomenon is marked when cupric chloride, cupric bromide, nickel bromide, and nickel iodide are dissolved in water to saturation. Cupric chloride is a blue salt not deliquescent or liable to absorb water from the air, but rather efflorescent, i. e. apt to part with its water of crystallization; hence, when the blue crystals are dissolved in water they form a deep green liquid, which on dilution becomes azure blue, this azure blue solution by heating again becomes green. The remaining salts are of a yellowish green tint, but on dissolving in as little cold water as possible they give intensely coloured brown liquids. Cupric bromide crystals effloresce, losing 10 per cent. of water, and becoming very dark brown, nearly as possible black in colour even in ordinary air, but in presence of moisture they are deliquescent.

Solution facilitates chemical change. Hence, when green cupric bromide crystals are dissolved in water the chemical attraction which binds the one molecule of metallic bromide to the five molecules of water of crystallization is so weakened that the cohesive attraction of the mass of the solvent water overcomes the chemical attraction of the crystalline water, and the resulting liquid is brown. When more water, however, is added a reversion of this action takes place, and the brown solution becomes green or blue.

IV. When a salt forms two distinct crystalline hydrates, A and B, at temperatures below 100° C., a saturated solution of A will, on heating to 100°, become a solution of B; at intermediate temperatures, a mixture of these two compounds.

This is inferred from the similarity in change of colour of a saline solution to the change of colour in the solid when heated. Bromide and chloride of copper, and, above all, iodide of cobalt, exhibit such changes.

The behaviour of this latter salt, which has been already mentioned, cannot possibly be explained except by the assumption that the hexahydrate, $CoI_2,6H_2O$, exists in the brown solution, and the dihydrate, $CoI_2,2H_2O$, in the green liquid produced from this by heat. The action of heat on cobalt chloride is not so well defined; that it is caused by dehydration we are certain, but whether the compound in solution is the dihydrate or the anhydrous chloride is a matter undetermined. It is not at all unlikely that the blue produced by alcohol is due to the formation of an alcoholate, for the spectrum, though very similar, is not quite identical with that of the hot aqueous solution. An experiment made on the hexahydrate was incidentally confirmatory of the action of water on the cupric chloride and bromide. When this salt was dissolved nearly to saturation in absolute alcohol the liquid had the usual magnificent blue colour; it was put aside under a bell-glass for crystals to separate, these appeared at the bottom of the deep blue liquid to be also of the same colour. When removed, however, and pressed between folds of paper, they had precisely the same composition and red colour as the original salt. Hence, though water could not on solution remove the water of crystallization from the salt, yet alcohol could do this, but on crystallization the original crystalline hydrate was formed, as is the case with cupric chloride and bromide.

V. The effect of heat on what are called the purple chromium solutions is not traced to dehydration, but to a distinct chemical reaction which yields a distinct class of compounds.

The violet or purple compounds of chromium have the general formula $Cr'''R_3 \cdot xH_2O$, or $Cr_2'''R''_3 \cdot xH_2O$; when solutions of these in water are heated to 100° C. there is formed a class of substances which may be written thus, $Cr_2'''OR_4 \cdot xH_2O$, and $Cr_2'''OR''_2 \cdot xH_2O$, or the first compounds may be said to contain the metal $Cr'''$ as the base of the compound, while the second contain the radical $Cr_2'''O$. This radical I would propose to call chromyl, just as UrO is called uranyl. And it may then be stated, that while the purple compounds are salts of chromium, the green are salts of chromyl.

This conclusion is derived more from the chemical behaviour of these solutions than from their optical properties. Thus green solutions having the same spectrum may be produced from the sulphate or nitrate—(1) by boiling a solution of the purple salt; (2) by dissolving an excess of chromic hydrate in the purple solution; (3) by neutralizing with an alkali one-third of the acid in a solution of the blue salts. 50$^{cc}$ of a boiled solution containing half a gramme of the sulphate or nitrate of chromium will not give a precipitate if boiled with two or three litres of water; but when one-third of the acid present has been previously neutralized, or when an excess of chromic hydrate is dissolved in the blue solution, a basic salt is thrown down on boiling with much water. This is exactly the behaviour characteristic of a salt described by Schrötter, to which he assigns the formula $Cr_2O_3 \cdot 2SO_3$, or which may be written $Cr_2O \cdot 2SO_4$. Lœwel describes a similar chloride, which is formed at 100° C.; this he writes thus, $Cr_2O_3 \cdot 4HCl$, and the formula may be modified in the following manner, $Cr_2O \cdot Cl_4 \cdot 2H_2O$. The modification has the following recommendation—that it explains the decomposition of the substance when heated to a high temperature, which Lœwel's formula does not. At or about 300° C. the compound is resolved into a mixture of the anhydrous violet compound of chromium and chlorine, $CrCl_3$ or $Cr_2Cl_6$, and the anhydrous green oxide, $Cr_2O_3$; if the original salt were a combination simply of hydrochloric acid with chromic oxide, heating would drive off the volatile acid and leave the oxide only, but the actual change indicates that the chromium and chlorine are in direct combination.

Having thus brought forward a new, if limited, application of spectrum analysis to chemical research, it is only necessary to add that the field of inquiry thus commenced is by no means exhausted. It has long been the writer's intention to add new facts to the study of organic chemistry by examining the invisible photographic spectra of colourless carbon compounds, but one pair of hands in the leisure hours of a not too long vacation cannot keep pace with one's ideas.

[W. N. H.]

## ANNUAL MEETING,

### Saturday, May 1, 1875.

WARREN DE LA RUE, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1874, testifying to the continued prosperity of the Institution and its efficient management, was read and adopted.

Sixty-four new Members were elected in 1874.

Sixty-two Lectures and Nineteen Evening Discourses were delivered in 1874.

The Books and Pamphlets presented in 1874 amounted to about 213 volumes, making, with those purchased by the Managers, a total of 347 volumes added to the Library in the year, exclusive of periodicals.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to the Professors, for their services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

PRESIDENT—The Duke of Northumberland, D.C.L.
TREASURER—George Busk, Esq. F.R.C.S. F.R.S.
SECRETARY—William Spottiswoode, Esq. M.A. LL.D. Treas.R.S.

**MANAGERS.**

William Bowman, Esq. F.R.S.
Adm. Sir Henry John Codrington, K.C.B.
Warren De la Rue, Esq. D.C.L. F.R.S.
The Duke of Devonshire, K.G. F.R.S. Chanc. Univ. Camb.
Sir T. Frederick Elliot, K.C.M.G.
Francis Galton, Esq. F.R.S.
The Hon. Sir Wm. Robert Grove, M.A. F.R.S. Just. C.P.
Cæsar H. Hawkins, Esq. F.R.S.
Alfred Latham, Esq.
The Lord Josceline W. Percy.
William Pole, Esq. M.A. F.R.S.
The Lord Rayleigh, M.A. F.R.S.
The Lord Arthur Russell, M.P.
C. William Siemens, Esq. D.C.L. F.R.S.
Sir Charles Wheatstone, D.C.L. LL.D. F.R.S.

**VISITORS.**

John Derby Allcroft, Esq.
John R. Andrews, Esq.
John Charles Burgoyne, Esq.
Sir Thomas Fowell Buxton, Bart.
Alexander John Ellis, Esq. F.R.S.
Thomas Hyde Hills, Esq.
Richard Liebreich, M.D. M.R.C.S.
William Longman, Esq. M.A.
A. C. Brisbane Neill, M.D.
John Rae, M.D. LL.D.
The Rev. Arthur Rigg, M.A.
John Bell Sedgwick, Esq.
James Spedding, Esq.
George Andrew Spottiswoode, Esq.
The Hon. John Gage Prendergast Vereker.

## GENERAL MONTHLY MEETING,

### Monday, May 3, 1875.

**George Busk**, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:

The Duke of Devonshire, K.G.
William Pole, Esq. M.A. F.R.S.
C. William Siemens, Esq. D.C.L. F.R.S.
George Busk, Esq. F.R.S. Treasurer.
William Spottiswoode, Esq. LL.D. F.R.S. Secretary.

George Critchett, Esq. F.R.C.S.
Constantine Ionides, Esq.
Mrs. Samuel Joshua,
Henry Joslin, Esq.
The Right Rev. Charles Perry, D.D. Lord Bishop of Melbourne,
Lieut.-Col. George Frederick Whitelocke,

were *elected* Members of the Royal Institution.

**John Tyndall**, Esq. D.C.L. LL.D. F.R.S.
was re-elected Professor of Natural Philosophy.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

*The Secretary of State for India*—Indian Trigonometrical Survey Synopsis, Vols. II. and III. 4to. 1874.
*Director-General of the Geological Survey*—Mineral Statistics for 1872 and 1873. 8vo. 1873-4.
Memoirs. 2 Parts. 8vo. 1873.
*Geological Survey of India*—Memoirs, Vol. X. Part 2; Vol. XI. Part 1. 8vo. 1873-4.
Records, Vol. VII. 8vo. 1874.
Palæontologia Indica, Ser. X. 1. fol. 1874.
*Agricultural Society of England, Royal*—Journal, Second Series, Vol. XI. Part 1.
Index to Second Series, Vols. I.-X. 8vo. 1875.
*Astronomical Society, Royal*—Monthly Notices, Vol. XXXV. No. 5. 8vo. 1875.
*Cazalet, W. C. Esq.*—Dr. J. F. Churchill; Consumption and Tuberculosis: their Treatment by the Hyposulphites. 8vo. 1875.

*Chemical Society*—Journal for April, 1875. 8vo.
*Clarke, Hyde, Esq. (the Author)*—Researches in Pre-historic and Proto-historic Comparative Philology, Mythology, and Archæology. 8vo. 1875.
*Dudgeon, P. Esq. (the Author)*—Historical Notes on the Occurrence of Gold in the South of Scotland. (M 9) 4to. 1875.
*Editors*—American Journal of Science for March and April, 1875. 8vo.
Athenæum for April, 1875. 4to.
Chemical News for April, 1875. 4to.
Engineer for April, 1875. fol.
Journal for Applied Science for April, 1875. fol.
Nature for April, 1875. 4to.
Nautical Magazine for April, 1875. 8vo.
Pharmaceutical Journal for April, 1875. 8vo.
Practical Magazine for April, 1875. 4to.
Telegraph Journal for April, 1875. 8vo.
*Franklin Institute*—Journal, No. 590. 8vo. 1875.
*Geographical Society, Royal*—Proceedings, Vol. XIX. No. 3. 8vo. 1875.
*Geological Institute, Imperial, Vienna*—Verhandlungen, 1874. No. 16. 8vo.
Jahrbuch, 1874. No. 4. 8vo.
*Linnean Society*—Journal, No. 79. 8vo. 1875.
*Manchester Geological Society*—Transactions, Vol. XIII. Part 10. 8vo. 1875.
*Medical and Chirurgical Society, Royal*—Proceedings, Part 40. 8vo. 1875.
*Meteorological Office*—Report of International Meteorological Congress at Vienna, 1874. 8vo. 1875.
*Preussische Akademie der Wissenschaften*—Monatsberichte: Register, 1859–73. Jan. 1875. 8vo.
*Roberts, W. Chandler, Esq. M.R.I.*—Society for Photographing Relics of Old London: Six Photographs of the Oxford Arms, Warwick Lane. April, 1875.
*Royal Society of London*—Proceedings, No. 160. 8vo. 1875.
*Smee, Alfred, Esq. F.R.S. M.R.I. (the Author)*—The Mind of Man: being a Natural System of Mental Philosophy. 8vo. 1875.
*Statistical Society*—Journal, Vol. XXXVIII. Part 1. 8vo. 1875.
*Sweetman, Walter, B.A. (the Author)*—Through the Night. A Tale, &c. 2 vols. 16to. 1869.
Poems. 2 vols. 16to. 1871–5.
Thoughts on Free-Will. (K 101) 8vo. 1873.
*Symons, G. J. Esq. (the Author)*—Symons' Monthly Meteorological Magazine, April, 1875. 8vo.
*United Service Institution, Royal*—Journal, No. 79. 8vo. 1875.
*University of London*—Calendar for 1875. 8vo.
*Victoria Institute*—Journal, No. 32. 8vo. 1875.
*Wurzburg Physikalisch-Medicinische Gesellschaft*—Sitzungsberichte, 1873–4. No. 1. 8vo. 1875.

## WEEKLY EVENING MEETING,

Friday, May 7, 1875.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Professor A. Cornu,

PROFESSOR AT THE POLYTECHNIC SCHOOL, AND COUNCILLOR OF THE PARIS OBSERVATORY,

### *On New Determinations of the Velocity of Light.*

The old philosophers and astronomers, until Galileo, thought that the propagation of light was instantaneous.

*Astronomical Determination of the Velocity of Light.*—Rœmer, a Danish astronomer, called to the Paris Observatory by the illustrious Picard, after having computed from some old observations the eclipse times of Jupiter's satellites, found great discrepancies between the calculated and observed times: the eclipses appear too soon when Jupiter approaches the earth and too late when it goes away. Rœmer ascribed these differences to the time necessary for the propagation of light, and concluded from his observations that light requires about eight minutes to come from the sun to the earth.

Bradley, one of the most illustrious English astronomers, seeking to put in evidence certain small annual motions of the stars caused by the displacement of the earth in space (annual parallax), found such a motion, but quite different from the expected one. The apparent deflection of the direction of a star—for instance, $\gamma$ Draconis, near the pole of the ecliptic—instead of being at every moment directed, as expected, towards the centre of the terrestrial orbit (the sun), is directed at a right angle. The greatest elongation (called *aberration*) rises to $40 \cdot 7''$ from six to six months. Bradley, after many attempts, ascribed this effect to the composition of the velocity of light with the velocity of the elliptic motion of the earth (1728).

From those observations, and from the approximate knowledge of the distance from the sun to the earth, the velocity of light was found equal to about 200,000 *English miles in a second*, in other terms, *one million times the velocity of sound.*

*Direct Determinations of the Velocity of Light* were for a long time considered as impossible, owing to the enormous value of this velocity. The first solution was given by M. Fizeau (1849) by the method of the *toothed wheel.*

Induced by some considerations analogous to the celebrated *accès theory* of Newton, M. Fizeau, one of the most illustrious members of the Paris Academy, and recently elected Honorary Member of the Royal Society, succeeded in rendering perceptible and even measurable the duration of the propagation of light for a distance of a few miles.

The principle of the method is the following:

A beam of light passes through the interval between two teeth of a rotative toothed wheel (*roue dentée*): this beam is reflected on a mirror fixed some miles distant, comes back exactly on the same line, and passes again through the same interval as before. An observer can receive this beam: he will see a luminous point, a *luminous echo*, through each hollow between two teeth: if the wheel revolves with an increasing speed the luminous impression will first become continuous. The wheel will soon revolve with sufficient rapidity to turn a small angle during the time necessary for the beam of light to go and come back again. The angular velocity can be so regulated that the solid part of a tooth is substituted for the hollow part during this time; then, on coming back, the beam will be obstructed by the wheel. The same obstruction will take place at each tooth, and the luminous echo will disappear.

If the velocity of the wheel be doubled, the luminous point will appear again, because the reflected beams will meet with the following hollow and pass through. With a triple velocity a new extinction will take place, as before.

The following apparatus is necessary to produce the exact reflexion of the beam:—At each station a telescope is directed to aim at the centre of the object-glass of the opposite station. The beam of light is sent through the first telescope: the pencil of rays, rendered nearly parallel, is received by the second and concentrated in its focus, and there reflected by a small mirror. After reflexion, the rays follow exactly the same path, and come back at the very point they start from. The observer can receive these *return rays* without being blinded by the source of light, by interposition of a piece of transparent glass, which reflects a good part of these *return rays*.

*M. Fizeau's Experiment* (between Suresne and Montmartre) was made to prove that it was possible, not only to establish the duration of the propagation of light, but also to measure its velocity without the intervention of astronomical phenomena. The distance of the stations was 8633$^{m}$, about 5½ English miles. The number found by M. Fizeau agreed sufficiently with the astronomical result to give the greatest confidence in the exactness of the method, when applied under fair conditions. A new experiment was arranged with Arago in the Paris Observatory, but Arago's death prevented the execution of this design.

*Professor Cornu's Researches.*—First experiments were made between the Polytechnic School in Paris and Mont Valerien. (Distance, 10310$^{m}$, about 6½ English miles.)

His researches were conducted with a view to improve the method of the toothed wheel, in order to obtain the greatest exactness. The chief difficulty for the practical application of this method is to measure the angular motion or velocity of the wheel, to which the velocity of light is directly compared. The simplest means would have been, as in M. Fizeau's experiment, to give an uniform motion to the wheel; but such a motion is practically impossible to obtain, so it was necessary to find another mode of measure. The principle of the new improvement was the use of an *electrical registering apparatus*, to register the continuous increase of motion of the wheel. With that arrangement an exact uniform motion is no longer necessary, the observer being able by a peculiar electric signal to point out the instant at which the right velocity is obtained.

The second improvement, and one very important for the exactness of the method, is the substitution of a pair of observations of the return rays, when reduced to a determined feeble intensity, for the single observation of a total extinction.

These improvements, experimentally tried in 1872, gave the velocity of light as 298,000 kilometres per second. The probable error does not rise to 1 per cent.

*Professor Cornu's New Determination* was made between the Paris Observatory and the tower of Montlhéry. (Distance, 22910$^{m}$, about 14½ English miles.)

A direct determination of the velocity of light was ordered at the beginning of 1874 by the Council of the Paris Observatory, on the proposal of M. Le Verrier, Director, and of M. Fizeau, Councillor. The best conditions were chosen for the optical and mechanical apparatus, and the stations were placed at an increased distance. One was erected upon the higher terrace of the Observatory, and supplied with a telescope of 0·38$^{m}$ (1¼ foot) aperture, and 9$^{m}$ (30 feet) focal length. The telescope and the remainder of the apparatus (toothed wheel, registering cylinder, clocks, &c.) were sheltered under a large cabin constructed on purpose. The opposite station was erected on the top of the tower of Montlhéry; it contains *only* a reflexion telescope sheltered by a cast-iron tube.

The experiments were made in the summer of 1874. The average of 508 pairs of observations gave the velocity as 300,400 *kilometres in a second of mean time.* The probable error appears not to exceed one-thousandth.

*Second Solution for Direct Measurement of the Velocity of Light* was obtained by the method of the revolving mirror (1850); based on the use of it by Sir Charles Wheatstone in his beautiful researches on the Velocity of Electricity (1834). Arago, after an enthusiastic account of these researches before the Paris Academy, showed how the new apparatus might be adapted to solve some most important problems of optics (1838), and specially to decide between the emission and the undulatory theory of light. He gave (April, 1850) a full description of his own attempts on the subject, but he was *not* able, through failing eyesight, to fulfil his design. Some days after, the complete solution of the problem was brought before the Academy simultaneously by Foucault and by MM. Fizeau and Bréguet. Foucault, in the year 1865, improved in several points the revolving mirror method, and obtained a direct determination of the velocity of light (298,000 kilometres).

The principle of the experiment is as follows:—

A beam of light reflected on a revolving mirror is normally reflected by a fixed concave mirror, and comes back again on the evolving one: during the time of the propagation of light from the first mirror to the second and back, the revolving mirror has suffered a little angular motion; the new reflexion on it produces a small deflection on the return beam; from that deflection the velocity of light can be computed.

This method is certainly one of the most curious, but the deflections are so small and the march of the rays takes place in such extraordinary circumstances that it is difficult to ascertain the degree of approximation of the result.

*Physical Importance of the Direct Determination of the Velocity of Light.*—The importance of the result is perhaps greater for those physicists who occupy themselves with electricity than for those who work on optics. The beautiful experiments and theories of Prof. Maxwell, Sir William Thomson, &c., so clearly expounded by the British Association Electrical Standard Committee, have shown that the velocity of light is a coefficient common to the undulatory waves, and to the mode of · motion which is called electricity. Several determinations, but purely electrical ones, have been made in England of that coefficient, and the results agree as well as possible, in that delicate matter, with the above-given value.

*Astronomical Importance of the same Determination.*—The numbers measuring the phenomena discovered by Rœmer and Bradley, combined with the approached distance of the sun to the earth, have a hundred and fifty years ago furnished an approximate value of the velocity of light. Now the progress of science requires an inverse march; the exact value of the velocity of light permits, by the inverted calculus, the computation of the mean distance of the sun or the sun's parallax, that is to say, the same element which is directly given by the transit of Venus. Professor Cornu's last result, combined with Delambre's equation of light (deduced from more than a thousand observations of eclipses of Jupiter's satellites) or of Bradley's aberration value, which seems one of the best determined number, agree exactly with the result obtained by M. Le Verrier in his researches on planetary perturbations, and with the already known results of the last transit of Venus observations.

[A. C.]

## WEEKLY EVENING MEETING,

Friday, May 14, 1875.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

John Evans, Esq. F.R.S. F.S.A. Pres. G.S. Pres. Num. Soc.

### *The Coinage of the Ancient Britons, and Natural Selection.*

The speaker began by saying that he feared that the title of the discourse might not at the outset appear to have that coherence between its two parts which is so desirable in the foundation on which a lecturer has to build. He hoped, however, that even by itself a slight sketch of the native coinage of this country, struck at a period when it had not as yet been brought under the Roman yoke, might not be without interest. If in addition he should be able to show that the succession of the types of the coins followed certain laws, to a great extent analogous with those by which the evolution of successive forms of organic life appear to be governed, the two subjects would no longer seem to be incongruous. Should he go on to show that a recognition of the successive stages of development in the coinage was absolutely necessary for its being properly understood, and for placing the various coins in even approximately chronological order, it would be seen that instead of a want of coherence there was, in fact, an intimate connection between the two portions of the subject.

Coins are excellent indiciæ of the state of civilization of any country. They exhibit the condition of the arts, especially with regard to the production of metals and metallurgy, and the skill of the engravers. They also illustrate the amount of commerce as distinct from barter, for the more numerous the denominations of the coins, the greater must have been the demand for them for the daily necessities of life; while their values and proportions, as compared with those of neighbouring countries, and the character of the types upon them, may illustrate the relations of a country to those around it. The devices on the coins also frequently serve to throw a light on the history and mythology of the countries in which they were struck.

Before proceeding to describe the coinage of ancient Britain, it was thought desirable to notice some of the early accounts of this country as given by Roman and Greek historians.

The intercourse between Britain and the more highly civilized nations of Southern Europe dates back to a remote period. So early as the middle of the fifth century B.C. Herodotus excuses himself for not being acquainted with the Cassiterides from which tin was brought to the Greeks. In the middle of the fourth century B.C. Aristotle makes mention of the Britannic Isles, Albion and Ierne; and again, in the second century B.C. Polybius apologises for not writing about the Britannic Isles and the working of tin. It is not, however, until the latter half of the first century B.C. that we obtain from Diodorus Siculus and Strabo, both of whom wrote shortly after the invasion of Julius Cæsar, some more detailed accounts of Britain, its customs and inhabitants. After giving some particulars of these latter, the speaker more especially called attention to the history of the commerce in tin, as given by these authors. Diodorus relates how, after it had been converted into ingots and carried to the island of Ictis—possibly St. Michael's Mount—it was purchased by merchants, who transported it to Gaul, through which country the tin was carried on horses by a thirty days' journey to the mouth of the Rhone. Strabo states that the tin and lead also were bartered for earthenware, salt, and bronze vessels, and that the traffic was carried on principally by Phœnicians from Cadiz. He it is who relates the anecdote of the Phœnician captain who, being followed by a Roman ship in order to ascertain the route to Britain, deliberately ran his vessel on a shoal, and received from the state the value of his cargo, in return for not having divulged what was then a national secret.

These relations have all the appearance of having been derived from some earlier sources, and are mainly important as showing the amount of intercourse there must have existed between the Continent and the extreme west of Britain.

With regard to the actual condition of Britain at the time of the invasion of Julius, B.C. 55, we must look to the account of the invader himself, and the supplementary notices of other authors. At that time a very considerable invasion of the warlike Belgic tribes from the Continent had taken place, driving into the interior those who by the tradition of the period were the indigenous race. In these Belgic districts the names of the towns were often the same as those of the continental homes of the settlers. The inhabitants were numerous; the buildings like those of Gaul; there was an abundance of cattle; and though some animals, such as hares, common fowls, and geese were not eaten, they were kept for amusement. The principal products which are noticed are corn, cattle, gold, silver, and iron, skins, slaves, and dogs, such as were used by the Gauls in war. Kent was the most civilized part, owing to its contiguity to the Continent.

In the interior the inhabitants did not sow corn, but lived on milk and flesh, though they did not know how to make cheese, incredible as this may appear to Welshmen. They had wives in common, and in many other respects there was a marked contrast between the tribes on the southern and south-eastern coast and those of the interior.

But though the former were so much more highly civilized than the latter, there is one remark of Cæsar which has caused many to doubt the existence of any coinage among them. The passage, as commonly received, is as follows: "Utuntur aut ære aut annulis ferreis ad certum pondus examinatis pro nummis." "They use either brass, or rings of iron adjusted to a certain weight, instead of money." It has, however, been pointed out by the late Mr. Hawkins that the passage is corrupt, and that in several MSS. there is express mention made of gold coins. But in addition to this, a letter to Cicero from his brother in Britain speaks of a tribute in money having been imposed upon the island "imperatâ pecuniâ"; and from other authors there seems reason to believe that a tribute was actually paid.

On any examination of the circumstances of the case, the absence of a coinage in Britain appears in the highest degree improbable. There was at the time of the invasion a remarkably close intercourse between Gaul and Britain. During the Gallic war a league of Gaulish tribes sent over to Britain for assistance. The news of Cæsar's intended invasion is reported to have been carried over to Britain by the traders; no less than four ports are mentioned by Strabo as those from which there was constant commerce to this country from the Continent, and Divitiacus is expressly mentioned as having held dominion over a considerable part of Britain as well as in Gaul. Other facts might be cited; but these suffice to show that if the use of coins was known in Gaul—and of this there is no question—it must also have been known in Britain. So far therefore as this passage is concerned we must regard it either as not containing the original statement of Cæsar, or else as being based on some misrepresentation of facts.

That this is the case is proved by the testimony of the coins themselves. Throughout the whole of the southern and south-eastern part of Britain, as far as a line joining the Severn and the northern part of Yorkshire—that is to say, in those parts which were once settled by Belgic tribes, and the adjoining districts—coins undoubtedly of British origin have been found in considerable numbers.

They are of two classes, uninscribed and inscribed, and in the names, types, and inscriptions on many of those of the latter class the influence of contact with the Romans may be traced. This class may, indeed, for the most part be attributed to the period between the second landing of Julius, B.C. 54, and the conquest by Claudius, A.D. 43-44, though some are of even later date.

During this interval of rather less than a century there was much intercourse between Britain and Rome, including, of course, the Roman province of Gaul. No doubt the annexation of Britain might have taken place at an earlier period than it did, had not Augustus purposely avoided extending the Roman Empire to Britain. More than once, however, he was induced to undertake the task, though he allowed himself to be diverted from it. On one of these occasions it was by British ambassadors, who deposited offerings in the Capitol, and are said by Strabo to have brought the whole island into friendly connection with the Romans. The Britons, indeed, are said to have agreed to pay duty on all exports and imports from or to Gaul, and this was considered preferable to garrisoning the island.

That Rome was here as elsewhere recognized as the mistress of the world was evinced by Adminius, a son of Cunobelinus the British chief, seeking refuge with Caligula, as did also one Bericus at the court of Claudius, and as had formerly another British prince at the court of Augustus.

It can therefore well be imagined that Roman art became known in Britain, and that probably artists trained either in Italy or Gaul settled in this country. It is certainly not surprising to find devices borrowed from Roman coins superseding or modifying the more purely native types.

Although numerous British tribes are mentioned by early historians, it is difficult in most cases to assign particular coins to them. This is partly owing to the names of the tribes but rarely occurring upon the coins, and partly to the difficulty in assigning exact limits to their territories, which, from conquests or other causes, appear to have been continually varying.

It has therefore been thought safer to treat of the coins under the head of districts rather than of tribes. The study of this class of coins is to a certain extent like that of geology; we have no written history on which to rely, and the annals of the past have to be reconstructed from the evidence of contemporary but dumb witnesses, disinterred from the soil. With gold coins, the geographical range of which is, as is well known, wider than that of those in the baser metals, it is unsafe to argue from only one or two instances; where, however, numerous coins of any given type have been found, we are able to fix their original home, and, by means of the types, to arrive at some conclusion as to the chronological order even of uninscribed coins.

The coinage of the several districts was next described, and diagrams of various specimens exhibited, some of which are reproduced in the accompanying * plate.

The inscribed coins of the western district, comprising Somersetshire, Gloucestershire, and Wilts, and parts of adjoining counties, give the legends BODVOC, CATTI, COMVX, VO-CORIO, EISV, &c., which are of doubtful interpretation. There are also coins of a prince ANTEDRIGVS, of which a specimen is engraved in Fig. 7.

In the south-western district of Hampshire, Sussex, and western Surrey, are found coins of Commius, an Attrebatian chieftain mentioned by Cæsar, and of two of his sons, Tincommius and Verica. Many of

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* The drawings are taken from Evans's 'Ancient British Coins,' 8vo, 1864, to the plates of which reference is also occasionally made.

these are executed in an artistic manner, and probably by foreign engravers.

The coins of a third son of Commius, Eppillus, are found in the Kentish district, which embraces Kent and eastern Surrey, as are also those of Amminus and Dubnovellaunus. This latter prince was among those who came as suppliants to seek aid from the Emperor Augustus, and his name is found under the forms DAMNO BELLAVNVS and ΔOMNΩN BEΛΛAYNOΣ on the celebrated monument of Ancyra. Figs. 23 and 29 show specimens of his coins.

In the central district, comprising Bucks, Beds, Herts, Middlesex, Essex, Northampton, and parts of neighbouring counties, numerous coins have been found. The principal are those of Andocomius, whose territory seems to have been in Bucks and Beds; Tasciovanus, whose capital was Verulam, now St. Alban's; and his son Cunobelinus —the Cymbeline of Shakspere—whose capital was Camulodunum, now Colchester. Coins of Tasciovanus and Cunobelinus are shown in Figs. 16 and 8, but there exists a vast variety of other types in gold, silver, and copper, many of which are distinctly copied from contemporary Roman coins.

The eastern district of Norfolk, Suffolk, and some part of the adjoining counties, appears to have been occupied by the Iceni. The coins are principally in silver, but there are gold coins of a chief, Addedomaros (Figs. 10 and 11), and also some uninscribed gold coins, such as Figs. 12 and 13.

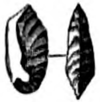
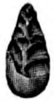
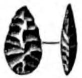
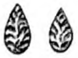
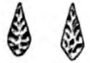
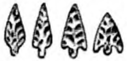
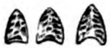
The Yorkshire district comprises parts of the adjacent counties to the south, and was mainly occupied by the Brigantes. The legends on the coins, VOLISIOS, DVMNO-CO VEROS; VEP. CORF, &c., have as yet defied interpretation. Some coins from this district are shown in Figs. 26, 27, and 28.

Taken as a whole, the series of inscribed coins bears witness to a considerable degree of civilization prevailing in Britain, especially in the parts most nearly adjacent to the Continent. Not only are there numerous and varied types skilfully engraved, but the different denominations of the coins show that the requirements of trade were by no means inconsiderable. Taking, for instance, the currency under Tasciovanus, we find a larger gold coin, weighing about 84 grains, and a smaller, representing one quarter of its value. There is but one denomination of silver coins, a small piece weighing about 20 grains. In copper, however, there are coins of three sizes, intended to weigh about 68, 34, and 17 grains respectively. So that about the Christian era there were current at Verulam, for the purposes of internal trade, no less than six denominations of coins.

The second class of coins, the uninscribed, occur also in the three metals, gold, silver, and copper, as well as occasionally in tin.

In many cases they appear to have been current at the same time as the inscribed coins, as they are found in the same hoards; but for the most part they appear to belong to an earlier date. In some parts of Britain, however, there does not appear to have ever

Plate I.

| | SIDE TOOL | TONGUE SHAPED | OVAL | LEAF SHAPED | LOZENGE SHAPED | BARBED | TRIANGULAR | |
|---|---|---|---|---|---|---|---|---|
| |  |  |  |  |  |  |  | |
| EARLY PALÆOLITHIC | ✚ | ✚ | ✚ | | | | | |
| LATE PALÆOLITHIC | ▮ | ▮ | ▮ | ✚ | ✚ | | | |
| EARLY NEOLITHIC | | | | ✚ | ✚ | | | |
| LATE NEOLITHIC | | | | ✚ | ✚ | ✚ | ▮ | |
| EARLY BRONZE | | | | ✚ | ✚ | ✚ | ✚ | |
| LATE BRONZE | | | | ✚ | ✚ | ✚ | ✚ | |
| IRON PERIOD | | | | | | | | |
| MODERN AUSTRALIAN | | | | ▮ | | | | |
| MODERN AMERICAN | | | | ✚ | ✚ | ✚ | ✚ | |

✚----DENOTES COMMON OCCURRENCE ▮----DENOTES RARE OR DOUBTFUL been an inscribed coinage. Among the gold coins, and not unfrequently among those in the other metals, there is a great general similarity of type on one or both faces. On the obverse there is usually a wreath in some form or other, and on the reverse a horse more or less distinctly portrayed; and this circumstance at once points to a derivation of the different varieties of type from some common source.

Two questions are therefore suggested: 1. What is this prototype or source? and 2. What are the laws which seem to govern its modifications?

Fortunately the prototype of many of the derivatives is readily to be recognized in the coins like Figs. 2 and 3 in the Plate, which are found over the whole of the south-east of Britain, though they would appear to be most abundant in Kent. The parentage of this British prototype can also readily be traced. There can be no doubt of its being a descendant from the Philippus—the gold *stater* of Philip II. of Macedon.

Nor is it difficult to see in what manner a type from so remote a country as Macedonia travelled to Britain. The close intercourse between that country and Gaul has already been mentioned, and in Gaul a coinage had existed for several centuries B.C. The knowledge of the art of coining seems to have been introduced from Massilia—now Marseilles—a Phocæan colony founded about B.C. 600, and from other Greek colonies founded in the south of France. The silver coins of Massilia were at an early period imitated in the surrounding districts, but until the latter half of the fourth century B.C. the currency of gold coins among the Greeks was limited in extent.

About B.C. 356, however, the gold mines of Crenides or Philippi were acquired by Philip II., and were soon made to yield an amount of gold equivalent to nearly 250,000*l.* per annum. An enormous extension of the gold coinage was a natural result, and the *staters* of Philip became current all along the coasts of the Mediterranean, and were soon seized on as objects for imitation by those who, like the Gauls of southern France, were on the borders of Greek civilization. The extension northwards through Gaul must soon have followed, and the transition thence into Britain was a natural consequence.

Though intermediate types exist between the Philippus, Fig. 1, and what is termed the British prototype, Figs. 2 and 3, it is needless to adduce them, the modifications which the types have undergone not preventing their original identity from being apparent, though they are by no means inconsiderable in extent.

On the obverse the laureate head of Apollo, or, as some have thought, of the young Hercules, has received additions in the shape of a bandlet round the head and a gorget on the neck, not improbably to give it the attributes of some more purely Gallic divinity. The wreath and hair have been reduced into a regular system, the back hair being rendered by a series of flowing locks all of one pattern, and the front hair by three open crescents. On the reverse, which originally commemorated the victories of Philip at Olympia, the charioteer has grown wings, but her body, forearms, and whip have become represented by a series of dots or pellets, the wheel of the chariot only at times survives, and the two horses have become amalgamated, though the legs are often bifid in remembrance of the original duality of the animal. All traces of the name ΦΙΛΙΠΠΟΥ have disappeared.

Starting with this British prototype, the next question is as to the laws which regulate its farther modifications, and how they are allied to those of natural selection. It is hardly necessary to enter at any length upon the theory of this form of evolution with which the name of Darwin will ever be associated. It is, however, desirable to give a brief summary of some of its principal features.

In successive generations of animals and plants, though the offspring as a rule absolutely resembles the parents, there is always a tendency to variation, so that forms in some degree varying from the parent form are from time to time liable to make their appearance.

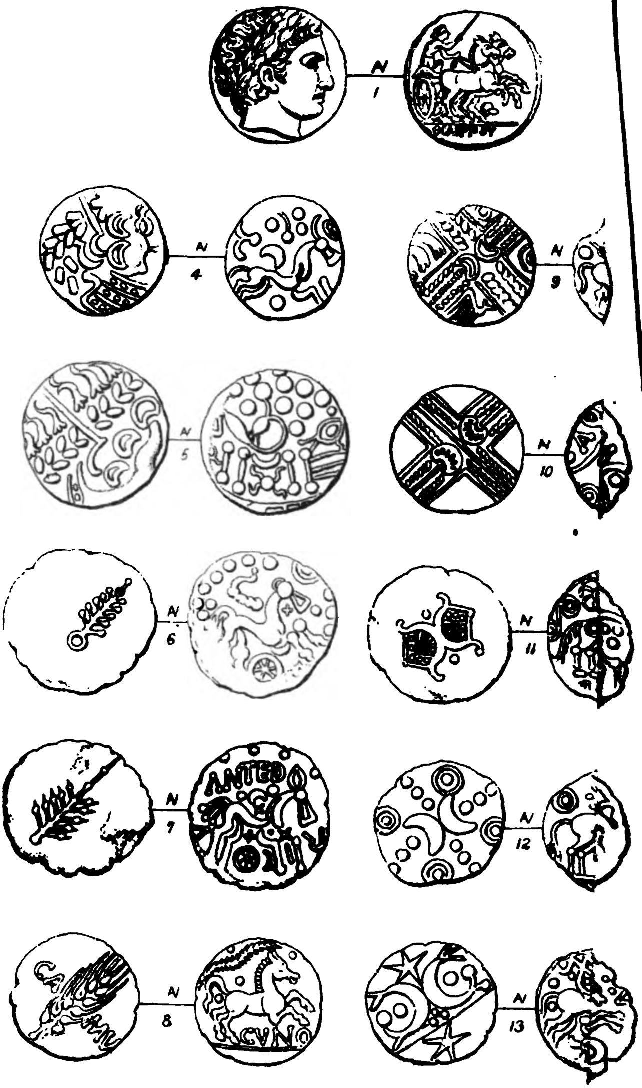
If in the struggle for existence these new varieties possess any advantage over the forms already in being, they are likely to be perpetuated, though for a length of time there is a liability in their progeny to revert in the direction of the earlier form.

Even long after some organ, owing to modifications in the mode of life, has ceased to be of service to the organism, it frequently survives in what is known as a rudimentary form.

Moreover, the degree in which a variety or modification is advantageous, and therefore likely to be perpetuated, is to a great extent dependent on external conditions, the changes in which afford opportunities for new forms to prove their utility.

No doubt this theory of descent with variation holds good with regard to most of the appliances of man; but it is more especially with regard to the changes in the types of coins, and in other ornamental devices among people in a low stage of culture, that we can trace laws somewhat analogous to those of natural selection. To the evolution of culture Colonel Lane Fox has devoted especial attention, and the present discourse is restricted to one special class of coins, though similar results to those which are found in Britain might be observed in the early coinage of some other countries.

The necessities of the case with regard to coins are: 1. That the successive issues or generations of coins should resemble each other sufficiently to pass current together; but, 2. Art being imperfect, there must have been more or less important variations and modifications in the successive dies which were engraved. This tendency to variation was increased by the necessity of the dies being rather larger in diameter than the coins which were struck from them, so that the new dies were often copied from coins not showing the whole of the device. They were also frequently copied from coins worn by circulation, and in many instances the external influence of the introduction of foreign artists made itself powerfully felt. 3. When not



EVOLUTION (

by this foreign element, the requirements which the types
fil in order to be perpetuated were, in addition to the neces-
mblance to the pre-existing types, *a*, that they should be of
cution ; and *b*, that they should present symmetry of form.

s well known that the natural instincts of man seem to lead
all parts of the world to the adoption of simple and symme-
forms of ornament ; while in all stages of culture the saving of
e and the production of the greatest amount of show at the least
le expenditure of labour, is an object of universal desire.

hus among the ancient Britons the reduction of a complicated
rtistic design into a symmetrical figure of easy execution, was
bject of each successive engraver of the dies for the coins, though
ably they were themselves unaware of any undue saving of
ble on their part, or of the results which ensued from it.

In devices modified in the manner suggested, it may well be
pected that we should find some portions of the original devices
viving in a merely rudimentary form, nor need we be surprised if
om time to time we find a tendency to revert in the direction of the
rototype.

The descent of some of the types of British coins was next traced by means of diagrams, some of which are reproduced in the accompanying Plate.

The principal features in the prototype, Figs. 2 and 3, are on the obverse, the wreath, the cross-band, the gorget, the back hair, which on Fig. 2 is arranged in one row instead of two, the front hair, and the face.

The latter, being difficult of execution and near the edge of the coin, is the first part to disappear, though it often survives in a rudimentary state as a mere swelling, like that on Figs. 4 and 5. Passing by some intermediate steps, it will be seen that by the time the stage represented by Fig. 14 is reached, the head has become reduced to a somewhat symmetrical pattern. The wreath is the main feature, but runs in two directions from a central boss ; the bandlet and the upper portion of the gorget form a cross line to the wreath. The back hair has been simplified into two large locks, one on each side of the bandlet ; and only one crescent of the front hair survives. In another stage, two crescents back to back have come to occupy the centre of the wreath, while the cross line has become developed into a second wreath at right angles to the first, and the whole device assumes the form of a cruciform ornament, in which the human head is difficult of recognition. On an examination of Fig. 9 it will, however, be seen that three of the angles of the cross are occupied by the gorget, some locks of the back hair, and a crescent representing the front hair, all in their proper positions. In Fig. 15 the artist has thought it sufficient to fill up each of the angles with locks of the back hair ; and in Fig. 16 the ornaments in the angles are a V-shaped representative of the gorget, and a crescent of the front hair, with pellets and annulets symmetrically arranged around them. Each of the four branches of the cross wreaths starts from a ring ornament, or a ring containing a pellet; and it seems probable that the artist who executed this cruciform device was unconscious of the fact that he was engraving the head of Apollo. When once this cruciform stage was attained, farther simplification became easy, of which examples are seen in Figs. 10 and 17. Fig. 18 gives another variety, in which the centre is occupied by a circular ornament instead of the crescents, and assists in showing how the ultimate descendant from the head, a sort of four-leaved flower, as seen on Fig. 19, was developed.

In other lines of descent the wreath retained its predominance, but there was still the tendency to develop a symmetrical ornament. This is illustrated in Figs. 20, 25, 26, and 27, the two latter of which show coins from the Yorkshire district. Another coin from the same district, Fig. 29, retains a small part of the original device, but little altered except by enlargement, so that three locks of back hair and a small portion of the wreath suffice to fill the field.

In another line of descent the wreath survived, modified in the same manner as on the coins with the cruciform devices, with the crescents either in the centre or at the side of the wreath, as in Figs. 23 and 24. In Fig. 29, copied probably from a worn coin, there is merely a raised band across the field, marking the position of the wreath; and in Fig. 22 the field is perfectly plain but convex, though the reverse is identical with that of Fig. 21, which shows the head on the obverse.

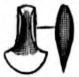
In another class of coins, which belong to the eastern district, the central crescents become the principal features. These are somewhat ornamented on the coin of Addedomaros with the cruciform type, Fig. 10, but form the whole device on another coin of the same prince, Fig. 11. On Figs. 12 and 13 other symmetrical modifications, with large crescents in the centre, are shown.

On other coins, as on those of the western district, the wreath alone is preserved, but in a form somewhat resembling a fern leaf (see Figs. 6 and 7). On the coins of Cunobelinus, when Roman art had attained great influence in Britain, it reappears as an ear of bearded corn.

On the reverses of the coins the fact of the device being a horse was rarely forgotten, although it came to be represented in a curiously disjointed manner, which saved the engraver much trouble. In Fig. 28 this conventionalized horse has its peculiarities perhaps carried to an extreme. In Fig. 5 another variety is shown, in which great importance seems to have been attached to the representation of the four legs. This form prevailed in Dorsetshire and the neighbouring counties. In most cases, however, when the artist who engraved the dies possessed sufficient skill, there was a reversion towards the original horse.

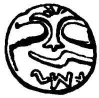
A remarkable metamorphosis of the horse is illustrated in the annexed woodcut. The upper figure shows a derivative from the type of the charioteer, which can be traced through 'Evans,' Pl. E, 9, and

Plate II.

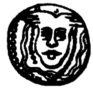
|  | FLINT. |  |  |  | BRONZE. |  |  |  |  |  |  |
|---|---|---|---|---|---|---|---|---|---|---|---|
|  | SIDETOOL | TONGUE SHAPED | OVAL. | CELT | CONVEX & FLAT SURFACES | STOP | FLANGE | STOP & FLANGE | OVERLAPPING FLANGE | BRONZE SOCKET | IRON SOCKET |
|  | | | | | | |  | | | | |
| EARLY PALÆOLITHIC | ✚ | ✚ | ✚ |  |  |  |  |  |  |  |  |
| LATE PALÆOLITHIC | ▮ |  | ▮ | ▮ |  |  |  |  |  |  |  |
| EARLY NEOLITHIC |  |  |  | ✚ |  |  |  |  |  |  |  |
| LATE NEOLITHIC -- |  |  |  | ✚ |  |  |  |  |  |  |  |
| EARLY BRONZE---- |  |  |  | ▮ | ✚ |  |  |  |  |  |  |
| LATE BRONZE--- |  |  |  |  | ✚ | ✚ | ✚ | ✚ | ✚ | ✚ |  |
| IRON PERIOD----- |  |  |  |  |  |  |  |  |  |  | ✚ |
| AUSTRALIAN------ |  |  | ▮ | ✚ |  |  |  |  |  |  |  |
| AMERICAN------- |  |  |  | ✚ | ✚ |  | ✚ |  |  |  |  |

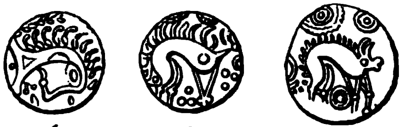
✚ DENOTES COMMON OCCURRENCE ▮ RARE OR DOUBTFUL

allied coins up to 'Evans,' Pl. D, Nos. 3 and 4, though several of the links are missing. When the type was placed before a Roman engraver it must have been difficult for him to understand its meaning. The general resemblance to a full face with wings above is, however, evident, and there can be but little doubt that some classical artist reproduced it as the head of Medusa on the coin engraved below. One thing is certain, that the coins of the two types are found together.



In another descendant of the Philippus not shown in the Plate, the wreath and hair become represented by a series of diverging lines, and from a coin of this kind it appears probable that the vine-leaf on the coins of Verica ('Evans,' Pl. II. 9) was derived. Such a complete change of the human head might appear incredible, but there are other instances. The close resemblance of the types of the first two coins in the annexed cut is evident, though the general device is in one case turned to the right and in the other to the left. The third coin shows that the device on the second is a boar, and on turning the page round at a right angle it will be seen that on the first is portrayed a human head in profile. These are silver coins from the eastern district.





It was thought needless to show the derivation of numerous types of coins belonging to the latter part of the series from Roman originals, and the bearing of the development of type upon the classification of the coins was next discussed.

As a rule those most distant from the prototype in character are the latest, though, of course, regard must be had to the locality to which the coins belong, as the districts nearest Gaul were exposed to different conditions from those more remote.

But, beside the succession of types, there is another important guide, which also results from a law that appears to be natural to all absolute governments which strike coins, if not indeed to other ruling powers. This is the great law of effecting economy at the expense of others, and is carried into effect by striking coins of the same denomination as those already in circulation, but either of less weight, or of baser alloy, or both.

In England we have been fairly free from the operation of this law, but our silver penny weighs but 7½ grains, instead of the real "pennyweight" of 24 grains. In Scotland, however, before the Union, the diminution in weight had been carried so far that the pound Scots was the equivalent of only one shilling and eightpence English. But for the successful application of this law the palm must be awarded to France, where the franc or livre, and its parts, the sou and denier, are the direct descendants from the same stock as our £. s. d.

We have already seen how in many respects the ancient Britons were far removed from that barbarism with which they are so commonly credited, and certainly they understood that part of the art of government which relates to a tampering with the currency, remarkably well.

The coins which have been termed the British prototypes are of remarkably fine gold, and weigh from 110 to 120 grains, the weight of the original Philippus being about 133 grains. Those which come nearest to this prototype, as, for instance, those of the types Figs. 4, 5, and 20, usually weigh from 95 to 100 grains. Coins like Figs. 14, 21, and 22 are commonly about 90 grains in weight. The coin engraved as No. 9 weighs about 86 grains, and by the time the cruciform ornament was established the weight was brought down to about 84 grains, at which point it became permanent, or nearly so, though the gold was reduced in purity to such an extent that many coins, like Fig. 27, and of analogous types, do not contain a quarter of pure gold.

It is evident that such a reduction as that from 120 to 84 grains could not have been sudden. It is far more probable that it was effected by gradual and almost insensible alterations, as is indeed suggested by the varying weights of the coins.

Taken in conjunction with the type, the diminution in weight may give some idea as to the time necessary for the changes in the coinage to have been effected. The date of the Philippus is known, as is also that of some of the ancient British coins; and we may either take the amount of time necessary for the degeneration of the Philippus to the form of the British prototype, or for the metamorphosis of this latter into some other type of ascertained date, or both these means, to assign an era for the commencement of the British coinage.

There are coins of Commius and his sons of much the same type as Fig. 21, but weighing only 84 grains; and as Commius was a contemporary of Julius, they probably date about 30 or 40 B.C.

Dubnovellaunus, again, some of whose coins are given in Figs. 23 and 29, was a contemporary of Augustus, whose protection he appears to have sought about B.C. 21 or 22.

Cunobelinus died about A.D. 40, and as his reign was of long duration, as was also that of his father, Tasciovanus, the date of B.C. 30 assigned for the commencement of the rule of Tasciovanus, is probably not far wrong. One of his early coins is shown in Fig. 16.

Taking a hundred and twenty years as the time necessary for the change in weight and type from the British prototype to the coins just mentioned, the date B.C. 150 would be assigned to the prototype. On the assumption that the Philippus was first initiated in the *south* of Gaul about B.C. 300, this would give a hundred and fifty years as the period for the change from Fig. 1 to Figs. 2 and 3.

If the reduction in weight took place at a uniform rate, and commencing with 133 grains in B.C. 300, came down to 84 grains in B.C. 30, the weight of the prototype, 120 grains, would be reached about B.C. 229; but this is probably too early. Looking, however, at the stage of evolution to which the coinage had attained within but a few years after the invasion of Julius, there can be no doubt of the use of money having been known in Britain long before his time.

In conclusion, a hope was expressed that the value of these coins as throwing light on one of the obscurest portions of the history of this country had been made evident, as also that in their interpretation, the recognition of some laws of development analogous to a great extent with those of natural selection, afforded no mean assistance.

Under any circumstances, the author trusted that a more favourable impression of our ancestors or predecessors in this country at the time of Cæsar's invasion had been created than that which commonly prevailed. He hoped they would no longer be regarded as the merest savages, "who stained themselves blue, sat under the mistletoe, and indulged in obscene rites," and who burnt the living in wicker baskets, instead of, in accordance with modern views, burying their dead in those useful articles, for the manufacture of which Britain was famous even in Roman times.

[J. E.]

## WEEKLY EVENING MEETING,

Friday, May 21, 1875.

William Spottiswoode, LL.D. Treasurer, Secretary, and Vice-President, in the Chair.

### James Baillie Hamilton, Esq.

### *On the Application of Wind to String Instruments.*

The Æolian harp is an instrument played upon by the wind uncontrolled by human agency. The sounds thus produced, though wild and fitful, are so impressive and peculiar, that when we hear similar sounds or produce such by accident or design, we immediately identify them and characterize them by the name Æolian.

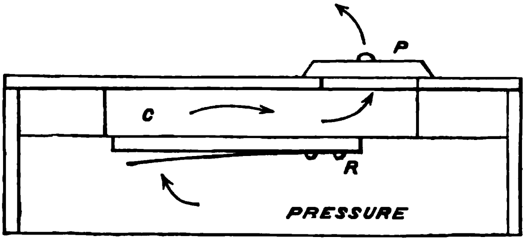
The well-known sounds thus produced, although useless for musical purposes, have long shown the capabilities of wind and strings, and have excited in many minds the desire to reduce these Æolian sounds to human control. In order to gain control the draught would be increased by a bellows or other artificial means, and concentrated upon the string by approximating the surfaces between which it lies, until nothing was left but a narrow slit, corresponding to the string's dimensions. Power and control would certainly be gained, but the intermittent closing of the slit by the string would produce only a reedy effect, and the pleasing features of the Æolian tone would be lost. If, however, this slit was reduced to a small portion of the string's length, the remainder of the string would be free from this defect, and subject to control by the finger or other means. The portion on which the wind impinged would, moreover, be flattened so as to offer more resistance to the draught.

These have been the steps generally adopted by all those who have attempted to gain any musical results from wind and string, and the means employed by them may be described as a string excited by a current of air concentrated on a flattened portion of its length, generally at or near the end.

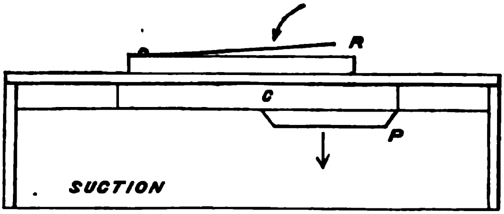
Many years ago Mr. John Farmer, our organist at Harrow School, substituted the tongue of a harmonium reed for this flattened portion of the string, and showed that the remainder of this compound string could be played upon like the string of a violin. Before the completion of an instrument a further knowledge of the laws affecting reeds and strings was of course essential. But Mr. Farmer will show to you this evening what can be done by substituting for a bow the action of wind upon a reed. And it is clear that as long as a reed and string are used in combination, whatever results may be achieved and whatever modifications be necessitated, Mr. Farmer must be regarded as their original author.

The reed plays such an important part in all experiments shown to-night that it may be well to describe at once the kind here employed. It is a "free reed," such as is used in harmoniums and kindred instruments. It consists of a tongue of metal vibrating from a root, and having a definite pitch. It lies over a frame which leaves a hole corresponding to the surface of the tongue, the fixed end of the tongue being riveted to this frame. If this tongue be plucked it affords a note of a certain pitch. This note is sustained in reed instruments by a draught of air; but in order that this draught may allow the vibration of the reed-tongue it is necessary to create true suction upon it by allowing some tube or channel to follow the reed-tongue. The mere frame of the reed acts as a rudimentary channel; and it will be seen on blowing a reed with the lips that the draught must always tend *towards* the frame, unless indeed the reed be preceded by some tube or cavity which allows of its vibration. But, as a rule, it is found more convenient to allow true suction to be formed by a channel *following* the reed. Accordingly, in harmoniums where pressure is employed, the air escapes from a common chest past the reed, and then through a channel. But when suction is employed, as in American organs, the air *enters* past the reed and its rudimentary channel into the general cavity. In both cases the pallet or valve succeeds the reed.

The following diagrams will help to explain the action of the draught on the reed in these instruments.



Harmonium where pressure is used.
*R*, reed. *C*, channel. *P*, pallet.

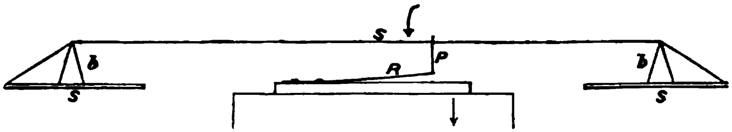


American Organ.

These drawings are only diagrammatic, as the modifications now used are very numerous.

The mode of connecting reed and string now adopted is a rigid pin passing from the free end of the reed-tongue to the string. The reed and string may thus be on different levels, and enjoy the conditions which suit them best respectively.

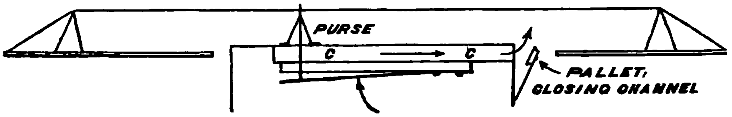
The following diagrams show the arrangements necessitated by this mode of connection.



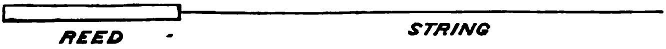
These drawings are only diagrammatic.

*s*, string.
*b*, bridge.
*S*, soundboard.
*P*, connecting-pin.
*R*, reed.

The arrows indicate the direction of the draught.



1. *Suction.* In this case no new arrangements are necessitated as regards the reed in order to allow its connection with the string. But in the case of pressure (2) it will be seen that a channel *intervenes* between reed and string, accordingly the pin passes *through* this channel, which is preserved air-tight by a "purse," which allows the required movement of the pin which it surrounds.



The original mode of connection may be thus represented, and illustrates most conveniently the way in which the reed enters into the constitution of the string so as to alter the intervals obtainable from it. Unlike a bow, whose weight does not affect the intervals of the string which it moves, the reed forms part of the string which it excites, and if a string were set at the same length and tension in conjunction with a succession of reeds, the proportional lengths corresponding to given intervals would alter with every reed applied.

[Large diagrams exhibited upon the walls were here referred to. The diagrams showed that, taking strings of different calibre and with uniform tension upon them, each string being 64 inches in length, the relations of lengths to musical intervals as in a free string plucked, were not maintained, thus:

| | The Third at | The Fourth at | The Fifth at | The Octave at |
|---|---|---|---|---|
| | in. | in. | in. | in. |
| A string giving at 64 inches the 16 feet C when connected with a reed of same pitch gave when shortened to .. .. | 51 | 48 | 40 | 23 |
| Similarly connected reed and string produced 16 feet E .. .. .. .. .. | 50½ | 47½ | 41 | 26 |
| Ditto, ditto, 8 feet G ♯ .. .. .. .. | 51 | 47 | 40½ | 22 |

Also showing that when under slackened tension the string divided into two segments, the free segment was always of greater length than the segment connected with the reed. In the instances above, measurements respectively were:

The free segment 34 inches as against reed segment 30 inches.
" " 35 " " " 29 "
" " 34½ " " " 29½ "

Every combination of reed and string having variable ratios according as the power of one or the other preponderates.]

Musicians will recognize these divisions as different from those of free strings. There is a point, however, more interesting than intervals, and that is, the qualities of tone resulting from this combination of reed and string. It is hardly necessary to speak of the numerous varieties of tone obtainable from either of these sources separately. The stops of harmoniums and reed organs show how many varieties of tone may be obtained from reeds with the most rudimentary channels and appliances; and the varied qualities of strings are equally remarkable according to their calibre, soundboard, and mode of excitation. But it might be supposed that the tones afforded by their combination must be either that of a string or reed, according as either element predominated. A single experiment will remove any such impression. Here is a very large reed combined with a very small and short string; also a small reed with a very long one: yet the former gives a pure clean tone indistinguishable from this organ pipe (an open metal diapason) which I try against it, whilst the latter, in spite of the predominance of the string, gives a comparatively coarse and reedy effect. The real varieties of tone and their causes are due mainly to the following simple facts, which may be expressed almost in a tabular form:

*In free reeds*, as a general rule, large excursions of the tongue tend to produce reedy, coarse, and striking tones; small excursions, more pure and soft and massive. For example, here are two reeds precisely similar, only the tongue of one of them is confined by a spring, so as artificially to render its vibrations of small amplitude: the difference in purity of tone is at once apparent.

*In strings*, flexibility promotes large excursions,—rigidity, small excursions; the flexibility or rigidity of the string being determined by its relative length, tension, or calibre. If a given reed were connected with a flexible string, and then with a rigid one, the result of the first combination would be a tone of the nature of a reed-pipe, and the second of a flue-pipe in an organ.

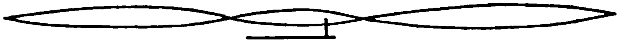
This is of course a very general description, for the gradations of flexibility of strings and of tones in these two great classes of organ pipes are in both cases very numerous.

But the main results may be illustrated by the following experiment: Here are two reeds affording by themselves, as you hear, a note of the same pitch. These are now attached to two different kinds of strings. The first is a thin wire a foot long, at a moderate tension, so that it comes under the head of flexible strings. The result is a note somewhat of the trumpet character, loud, brilliant, and reedy. The other and similar reed is attached to a wire at the same tension, which, however, is three feet long, and six times the calibre of the other. Though this is also a flexible string, the result is different, for the weight of the string seems to regulate the vibrations of the reed very much according to its own natural movement; consequently the excursions of the reed are in no way exaggerated, and the result is a decided string tone, free from all reedy character. Again, here are two strings affording a similar pitch; they are both a foot long, and of the calibre of a pianoforte string about the middle of the scale. They are both at a somewhat high tension. To the first is attached a large reed of a very low pitch, many octaves below the note of the string, and that of the combined tone now afforded, which is of a soft, massive character, such as characterizes the flue-pipes of an organ.

In this case the naturally large and powerful vibrations of the reed are simply confined and transmitted to the soundboard by the small rigid string. The tone of the reed is thus completely purified, and the displacement of air which its naturally large excursions would have caused, is far more than replaced by the action of the soundboard. The other similar string is attached to a reed far nearer to its own pitch, whose tone it purifies and intensifies in the same way. But as there is a more perfect sympathy existing between their natural note of vibration, the result is a note equally pure, but more rich and cloying, differing from the other as the note of a French horn differs from that of a stopped flue-pipe. On these simple causes four main sources of variety of tone depend.

All these experiments are made under the same conditions of reed channel and soundboard. But when it is remembered that any one of them can be modified as it stands, by the simple treatment which affects reeds, and the arrangement of soundboard which affects strings, it will be seen that any one of them can be multiplied by the causes that affect either reeds or strings so as to afford distinctive qualities, and that these qualities may be further combined and merged into each, so as to afford all the gradations between trumpet and horn, and flute and voice and string.

In all these experiments, and in the wind violin, the string combined with the reed vibrates in a single segment. But if the string were prolonged on either side it would resolve itself into three segments. Here is such a note; on comparing it with this diapason pipe, its sweetness and intensity are apparent. It will be seen that in this case the string is thus resolved, the nodes and segments being very clear and well defined.



The two terminal segments are equal, whilst that to which the reed is attached is shorter.

This resolving of the string into segments is now unnecessary for the production of a good tone. But it had at one time an important influence upon the development of reed and string, both as affording encouragement by the tones obtained, and by indicating optically the direction in which results were to be looked for. For there was a time when the only results that could be shown were a few notes produced violin fashion on a single string. And before liberty and means could be gained for developing Mr. Farmer's principle, it was necessary that something essentially remarkable as sound should be produced. In the search for this, the best results always occurred when the string was thus resolved, and for it a perfect sympathy of reed and string was thus secured, which the most deliberate arrangement can hardly surpass when one segment alone is used.

The notes thus obtained produced the desired effect, and many celebrated musicians and savants urged the prosecution of experiments. Sir Fred. Ouseley was one of the first to declare that as far as sound went the matter was worth prosecuting to the uttermost. Lord Lindsay then kindly set his laboratory at the disposal of the undertaking, and it has been there that notes obtained by a kind of natural coincidence, and slow of speech and difficult to control, have become systematized deliberately and rendered prompt and simple in their action. And if reeds and strings fulfil what they seem to promise, it will be due to the generosity and patience of many men.

Before explaining the musical results obtainable from reed and string instruments, it may be as well to deal with some difficulties which may have been anticipated.

When used as a wind violin, no difficulties present themselves except that of arranging the strings and intervals so as best to suit the performer. But when used in a rigid instrument, two difficulties at once suggest themselves: 1st, the notes might not respond to the touch; 2nd, they might go out of tune.

As regards the first difficulty, anyone is welcome to satisfy himself. And it may seem remarkable that the most rigid strings ensure as a rule the most perfect promptness, and the better the note the quicker its speech, any delay being due to a want of that sympathy between string and reed, which necessarily requires long study to establish.

But it might naturally appear that an instrument partially composed of strings might share their liability to alter in pitch. At one time it was determined to obviate this by using a string which should be insensible to changes at tension within certain limits.

It may be noticed that an elastic band extended and plucked does not alter its pitch until its elasticity is nearly exhausted by tension, when it alters rapidly. Here is an elastic string in metal forming apparently a solid bar, which is really a flattened steel band thrown into a helical form. On tuning it and striking it with a hammer, it may be compared with this pianoforte string beside it. The tension which affects the solid string by nearly an octave hardly alters the spiral string a semitone. Moreover, a short length of such a string might be attached to a solid wire and lie beyond the bridge so as to act as a compensator. This would correct all structural displacement. But as a matter of fact, the strings now used are so short, the framework so compact, and the tension on the whole series so low, that structural displacement need no longer be considered. Next, strings may be affected by temperature; but there are metals and alloys which are practically unaffected. And although these could not be hitherto used in pianofortes and instruments which depend on the free string for their sound, yet, in this case, the substance of the metal wire which conveys the reed's vibrations is immaterial.

Another source of derangement in string instruments hitherto, has been the gradual exhaustion of the string's retractile power by its own constant vibration, and especially by the shock conveyed in causing it so to vibrate.

Now when a reed is the motive power there is no such shock; and as has been already shown, the amplitude of excursion is reduced to a minimum. Moreover, the reed is capable of holding its own against the string, as much as the string of influencing the reed. Supposing a reed and string in combination to afford notes of varied quality, prompt in speech, easy to control, and simple in arrangement, what form will musical instruments so constructed assume, and what position will they occupy in music?

When it is desired to gain the same control over a string worked by a reed as the violinist gains over a string worked by a bow, the strings are set before the performer, at a convenient level, and subject to the control of both hands, as in a zither. Wind is supplied to the reeds by a small feeder, as in a harmonium. It is admitted to each reed by a valve, which is opened by the same pressure of the hand which controls the string thus sounded. For a strip of wood which opens the valve by a "roller action" may run parallel to each string in the position naturally occupied by the thumb of the hand which controls it. The "feet" thus supply the place of the "bow," both hands being left free to control the strings. The quality of the sound can be any of the numerous ones which string and reed can supply; and Mr. Farmer has shown us that the "glide" and expression and control of the string, which can only be obtained from the violin by long practice, are here within the reach of the most unpractised hand. But when these sounds are controlled by a "key" we should consider rather the application of strings to wind instruments and the application of wind to strings.

The introduction of the "free reed" into Europe has brought within everyone's reach wind instruments deficient only in these very respects, in which string and reed instruments excel.

Anyone who examines a harmonium or reed organ will see that all that is unsatisfactory in them is due to the want of a real reinforcement to each note. All that the most costly arrangement of tubes or resonators can supply is effected the moment that a reed is connected with a string, which not only entirely purifies its sound by confining its vibrations, but transmits those vibrations to a soundboard, and thus reproduces the work of the reed in a form which is pure, massive, and voluminous, and eminently suited to support the human voice, as is the case with all sounds due to the action of a string and soundboard. As the space occupied by each string need not exceed the scale of each note on keyboard or pedals, it is hardly necessary to point out the economy of this system. A "stop," or series of such notes of a certain quality, naturally assumes a somewhat harp-like form. The wind is admitted to the reed of each string by a tracker or rod connected with its key.

Though it has been shown that such a string can perfectly imitate a diapason pipe, these strings need not be expected to supplant pipes in an organ, but rather to supply certain tones desirable in an organ which it is at present difficult or impossible for pipes to afford, especially in the lower part of the scale, where a large column of air is required to reinforce every note. Accordingly it is in doing what it is inconvenient or impossible otherwise to do that wind and string must earn its place in music.

I cannot conclude without once more reminding you that whatever results may be obtained, and whatever credit earned in developing the capabilities of reed and string, these experiments were instituted upon Mr. Farmer's original invention, and to him we all owe our thanks.

[J. B. H.]

## WEEKLY EVENING MEETING,

Friday, May 28, 1875.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

## Col. A. Lane Fox, F.S.A. M.R.I.

PRESIDENT OF THE ANTHROPOLOGICAL INSTITUTE.

### *On the Evolution of Culture.*

If we accept the definition of the term science as "organized common sense," we necessarily reject the idea of it as a "great medicine" applicable only to particular subjects and inapplicable to others; and we assume that all those things which call forth the exercise of our common sense are capable of being scientifically dealt with, according as the knowledge which we pretend to have about them is based on evidence in the first place, and in the sequel is applied to the determination of what, for want of a better word, we call general laws.

But in using this term law, we do not employ it in the sense of a human law, as a regulating or governing principle of anything, but merely as deduction from observed phenomena. We use it in the sense of a result, rather than a cause of what we observe, or at most we employ it to express the operation of proximate causes; and of the ultimate causes for the phenomena of nature we know nothing at all.

Further, in this development of the principle of common sense it has been said that the inductive sciences pass through three phases, which have been termed the empirical, the classificatory, and the theoretical.

Of these, the first or empirical stage may be defined as representing that particular phase of unorganized common sense in which our knowledge is simply a record of the results of ordinary experience, such as might be acquired by any savage or uneducated person in his dealings with external nature.

But as this condition of knowledge might perhaps be denied the claim to be considered scientific, it might be better perhaps to extend the term so as to embrace all that can be included under a practical knowledge of the subjects treated, in which these subjects are studied for their own sakes, or on account of their practical uses to man, and not with a view to generalizing upon them.

In this way it may be said that agriculture represents the empirical or practical stage of botany; mining that of geology; hunting and the domestication of animals that of zoology; the trade of the butcher that of anatomy; navigation by means of the stars that of astronomy.

Passing now over the boundary line which separates what are generally recognized as the physical sciences from the science of culture, in which the subjects treated are emanations from the human mind, we find that these also have their corresponding phases of development.

Commencing first with the science of language, which has been the earliest and perhaps the most important branch of human culture the study of which has been scientifically treated as yet, we find that Professor Max Müller, in the series of lectures delivered in this Institution in 1861–63, has shown that the science of language has its corresponding empirical or practical stage, in which it is studied only for its own sake, or for its utility as a means of intercommunication; not as a means of generalizing upon language as a whole, but merely for the purpose of understanding the particular languages which we wish to make use of in our intercourse with others.

In like manner passing from language to the particular department of culture which, for the reasons to be explained hereafter, I shall make the subject of this discourse, viz. the material arts, I shall endeavour to show that there exists also in relation to them a practical or empirical stage, which is the stage that we are now in with respect to them, in which we may include the whole of the constructive arts of mankind from the simple flint knife to the most complex machine of modern times when viewed from the standpoint of the mechanic or the artificer, not as subjects for generalization, but merely from an utilitarian point of view.

There are many persons no doubt who regard utility, not as a primary stage, but as the final and highest result of science. But the highest achievements of science, even the highest practical achievements, would never have been reached by the mere utilitarian. There is a force within us by which we are moved in the direction of acquiring knowledge for its own sake and for the sake of truth, regardless of any material advantage to be derived from such knowledge. Sooner or later such knowledge is sure to bear practical fruits, even though we may not live to realize them.

It is in this spirit that men of science have advanced to the second or classificatory stage, in which, with a view to higher generalization, the subjects studied are grouped together according to their affinities, and specific points of resemblance are taken as the representatives of each class.

These classes are at first grouped round independent centres; but such an arrangement of them, having no existence in reality, is purely subjective and can only be transitional. The margins of the classes so formed represent only the margins of our knowledge or our ignorance, as the case may be.

By degrees, as the classes become extended, sub-classes are formed, and they are seen to arrange themselves in the form of branches radiating from a central stem. By still further observation the stems of the several classes are seen to tend towards each other, and we are led to trace them to a point of union.

Thus from the classificatory or comparative we pass gradually into the third stage, which I have spoken of as the theoretical, but which may perhaps be more clearly defined as the evolutionary. By the use of this term evolutionary we make it apparent that our third stage is but a development of the second, evolution being merely the necessary and inevitable result of the extension of classification, implying greater unity and broader generalizations.

These three stages then, the empirical or practical, the classificatory or comparative, and the evolutionary, are applicable to the development of all the inductive sciences.

But it has been held by some that a broad line of demarcation must be drawn between the physical sciences properly so called, such as zoology, botany, and geology, which deal with external nature, and those sciences which have been termed historic, which deal with the works of man.

This question has been ably treated by Professor Max Müller in the series of lectures to which I have referred, a course of lectures which must be regarded as a starting point and basis of instruction for all who follow after him in the same path.

But in claiming for the science of language, and for language only, a place amongst the physical sciences, he has made admissions to opponents which, in my humble judgment, ought not to be made, and which are inconsistent with that more extended view of the subject by which I contend that, if language, then all that comes under the head of culture must be included amongst the physical sciences. Thus, for example, we find him admitting this passage as a sound and reasonable argument on the part of those who deny the claim of language to be included amongst the physical sciences: "Physical science," he says, "deals with the work of God, historical science with the works of man."

Now if in dealing with what are here termed the historical sciences, we were to take the subjects of such sciences, as for example the arts or language, implements or words, and were to regard them as entities to be studied apart from their relation to mind, and were to endeavour to deduce from them the laws by which they are related to each other, it is evident that we should be dealing with a matter which could not be correlated with the physical sciences; but such a course would be absurd. It would be as absurd to speak of a boomerang as being derived by inheritance from a waddy, as to speak of a word in Italian being derived by inheritance from a corresponding word in Latin; these words and these implements are but the outward signs or symbols of particular ideas in the mind; and the sequence, if any, which we observe to connect them together, is but the outward sign of the succession of ideas in the brain. It is the mind that we study by means of these symbols.

But of the particular molecular changes or other processes which accompany the evolution of ideas in the mind, we know no more than we do of the particular molecular changes and other processes which accompany the evolution of life in nature, or the changes in chemistry.

If then we are to understand the expression "the work of God" as implying the direct action of ultimate causes, it is evident that we are not in a position either to affirm or to deny or to make any statement whatever respecting such ultimate causes which may operate either as directly or as indirectly in the one case as the other. We know nothing about them, and therefore to invoke ultimate causes as a reason for distinguishing between the sciences is to take up a position which cannot be scientifically maintained.

With equal if not greater truth we may combat the assertion that the science of culture is historical, whilst nature, on the other hand, as dealt with by the physical sciences, is incapable of progress. However valid this objection might have appeared during the empirical and comparative stages of the physical sciences, it cannot be maintained, since the researches of Darwin and others have fairly landed them in their evolutionary phase. The principles of variation and natural selection have established a bond of union between the physical and culture sciences which can never be broken. History is but another term for evolution. There are histories and histories, as anyone may determine who has read Green's 'History of England,' and compared it with the kind of matter which passed for history in his school days. But our position with regard to culture has always been one which has forced on our comprehension the reality of progress, whilst with respect to the slow progress of external nature, it has been concealed from us, owing to the brief span of human existence and our imperfect records of the past. The distinction, therefore, between the sciences, as historical and non-historical, is but a subjective delusion, and not an objective reality; and herein, I believe, lies the secret of most of those errors that we have to contend with.

But the point in which I venture more particularly to differ from the conclusions of the learned author of the Science of Language is the line which he has drawn between language and the other branches of culture by including language amongst the physical sciences whilst he excludes the rest. "If language," he says, "be the work of man in the same sense in which a statue, a temple, a poem, or a law, are properly called works of man, the science of language would have to be classed as an historic science;" and again he says, "It is the object of these lectures to prove that language is not a work of human art in the same sense as painting, or building, or writing, or printing."

In dealing with this question it is material, as regards the relative claims of language and the arts to be studied as physical sciences, to distinguish between the general and the particular. If it is said that language as a whole is not a work of human design, the same may with equal truth be said of the arts as a whole. A man who constructs a building, a tool, or a weapon, can no more be said to have devised a scheme of arts, than the introducer of a new word can be said to have invented a language; but each particular word bears the impress of human design as clearly as a weapon or a coin. A word may be said to be a tool for the communication of thought, just as a weapon is an implement of war.

But, says Professor Müller, "art, science, philosophy, religion, all have a history; language or any other production of nature admits only of growth." But unless it can be shown that words are entities having the power of generating and producing other words, which arts, tools, or weapons, do not possess, the word growth can only be applied figuratively to language as it is to the arts, and in that case growth and history are synonymous terms. But this is absurd. Words, as I said before, are the outward signs of ideas in the mind, and this is also the case with tools or weapons. Words are ideas expressed by sounds, whilst tools are ideas expressed by hands; and unless it can be shown that there are distinct processes in the mind for language and for the arts they must be classed together.

But it is said, "language has the property of progressing gradually and irresistibly, and the changes in it are completely beyond the control of the free will of man. This, however, can only be accepted relatively. We know that in certain phases of savage life the use of particular words may be tabooed in the same manner that the use of particular implements or weapons may be tabooed; but it would be quite as hopeless for any individual to attempt to change the entire course of the constructive arts as to change the form of a language; the action of the individual man is limited in both cases to the production of particular words or particular implements, which take their place like bricks in a building.

Man is not the designer in the sense of an architect, but he is the constructor in the sense of a brickmaker or a bricklayer.

But the difficulty of tracing fleeting words to their sources operates to a great extent in effacing the action of the individual in language. Words become public property before they are incorporated in a language. It would be difficult to establish a system of patents for new words. Here again we see that the line drawn between language and the arts is a subjective delusion, not an objective reality. It is not true that words do not originate with individual men, but merely that we do not perceive it.

Modifications of words, like modifications in the forms of the arts, result from the succession of ideas or other causes affecting particular minds. They obtain acceptance through natural selection by the survival of the fittest.

The chance which a new word or a new implement has of surviving depends on the number of words or implements to be superseded, on their relative importance to the art or the language, and the persistency with which these superseded words or implements are retained. The truth of this is seen in the fact that vocabularies change far more rapidly than grammatical forms; because the same grammatical terminations are employed with a large number of different words, and they are therefore a more constant necessity of speech.

Hence early and barbaric languages may be connected by their grammatical forms long after their vocabularies have entirely changed. The same truth is seen in the fact admitted by philologists, that in small communities new words and modifications of words gain more ready acceptance than in large communities; because the struggle of the new words for existence is less in small than in large communities, and the dialects therefore change more rapidly. And the same causes influence the transformations which take place in the arts. Objects in common use change more slowly than those which are but little employed; the difference is merely one of degree and not of kind.

In dealing with the arts, each separate contrivance occupies a larger share of our attention, to the exclusion of any comprehensive survey of them as a whole. The arts present themselves to our mental vision on a larger scale, and we view them analytically; we are as it were in the brickmaker's yard seeing each brick turned out of hand, whereas in dealing with language we see only the finished building, the details are lost. We view language synthetically. The arts may be said to present themselves to us as a sea beach in detached fragments; language in the form of a compact sandstone. The empiric or the utilitarian may deny that there is any resemblance between them; but the geologist knows that the mode of deposition has been the same in both cases, and he classes the whole as rocks.

Then again there are facilities for collecting and arranging the data for the study of language which do not exist in the case of the arts. Whilst words take seconds to record, hours and days may be spent in the accurate delineation of form. Words cost nothing, may be packed in folios, transmitted by post, and stored on the shelves of every private library. Ten thousand classified words may be carried in the coat pocket without inconvenience, whilst a tenth part of that number of material objects require a museum to contain them, and are accessible only to a few: this is the reason why the arts have never been subjected to those classifications which form the groundwork of a science.

But when we say that words and implements are both tools employed for the expression of thought, it is important to bear in view one difference between them, which has a practical bearing on the relative value of the two studies as a means of tracing the evolution of culture in prehistoric times and amongst savages. The word is the tool of the ear, the implement the tool of the eye; and for this reason language is the science of historic times, whilst the arts constitute the subject of science to be studied in relation to prehistoric times.

Every new tool or weapon formed by the hand of man retains the same form as long as it continues to exist; it may be handed from man to man, from tribe to tribe, from father to son, from one generation to another; or buried in the soil, it may under special conditions continue for untold ages without change of form, until in our own time it may be discovered and employed as evidence of the condition of the arts at the time it was fabricated. Very different, however, is the history of words. Each word coined by the exercise of the inventive faculty of man to express an idea is liable to change as it passes from mouth to ear. Its continued identity is dependent solely on memory, and it is subject to phonetic and acoustic changes from which the forms of the arts are exempt.

When by the invention of writing each word receives its equivalent in forms that are appreciable to sense of sight, it gains stability, which places it on a footing of equality with the arts, and enables us to trace with certainty the changes it has undergone; and therefore in historic times language is the surest test of social contact that we can have. But in prehistoric times, before it has acquired this permanence through the invention of writing, the forms of language were, to use Mr. Sayce's expression, in a constant state of flux.

The truth of this is seen in the immense number of dialects and languages employed by savages at the present time. Thus amongst the one hundred islands occupied by the Melanesian race, the Bishop of Wellington tells us, and his statement is confirmed by the late lamented Bishop Patterson, that there are no less than two hundred languages, differing so much that the tribes can have but very little interchange of thought; and similar accounts are given of rapid changes of language in Cambodia, Siberia, Central Africa, North, Central, and South America.

The greater stability of the material arts as compared with the fluctuations in the language of a people in a state of primæval savagery, is well shown by a consideration of the weapons of the Australians, and the names by which they are known in the several parts of that continent. These people, from the simplicity of their arts, afford us the only living examples of what we may presume to have been the characteristics of a primitive people. Their weapons are the same throughout the continent; the shield, the throwing-stick, the spear, the boomerang, and their other weapons differ only in being thicker, broader, flatter, or longer, in different localities; but whether seen on the east or the west coast, each of these classes of weapons is easily recognized by its form and uses. On the other hand, amongst the innumerable languages and dialects spoken by these people, it would appear that almost every tribe has a different name for the same weapon. The narrow parrying-shield, which consists of a piece of wood with a place for the hand in the centre, in South Australia goes by the name of Heileman, in other parts it is known under the name of Mulabakka, in Victoria it is Turnmung, and on the west coast we have Murukanye and Tamarang for the same implement very slightly modified in size and form. Referring to the comparative table of Australian languages compiled by the Rev. George Taplin, in the first number of the 'Journal of the Anthropological Institute,' we find the throwing-stick, which on the Murray River is known by the name of Yova, on the Lower Darling is Yarrum, in New South Wales it is Wommurrur, in Victoria Karrick, on Lake Alexandrina Taralye, amongst the Adelaide tribes, South Australia, it is Midla, in other parts of South Australia it is called Ngeweangko, and in King George's Sound Miro.

From these considerations we arrive at the conclusion that in the earliest stages of culture the arts are far more stable than language, whilst the arts are subject only, or chiefly, to those changes which result from growth; language, in addition to those which result from growth, is also affected by changes arising from phonetic decay.

The importance therefore of studying the grammar, so to speak, of the arts becomes apparent, as it is by this means alone that we can trace out the origin and evolution of culture in the earliest times.

The task before us is to follow by means of them the succession of ideas by which the mind of man has developed, from the simple to the complex, and from the homogeneous to the heterogeneous; to work out step by step, by the use of such symbols as the arts afford, that law of contiguity by which the mind has passed from simple cohesion of states of consciousness to the association of ideas, and so on to broader generalizations.

This development has to be considered under the two heads of culture and constitution, that is to say, that we have to consider not only the succession of ideas in the mind resulting from experience, but also the development by inheritance of the internal organism of the mind itself, or, to use the words of Mr. Herbert Spencer, "In the progress of life at large, as in the progress of the individual, the adjustment of inner tendencies to outer persistencies must begin with the simple and advance to the complex, seeing that both within and without, complex relations, being made up of simple ones, cannot be established before simple ones have been established."

We find no difficulty in assenting to the general proposition that culture has been a work of progress. Our difficulty lies in realizing the slow stages of its early development, owing to the complexities both of our mental constitution and of the contemporaneous culture from which experience is drawn, or, again to use Mr. Spencer's more expressive words, "of our inner tendencies and outer persistencies;" we are apt to regard as intuitive, if not congenital, many simple ideas which in early culture can only have been worked out through the exercise of experience and reason during a long course of ages.

We see this error of our own minds constantly displayed in the education of children. The ideas in a child's mind, like those of mankind at large, are necessarily built up in sequence. The instructor makes use of some word, the meaning of which is clearly understood by him, but which does not fall into the sequence of the child's reasoning; the conception associated with it in the child's mind must, however, necessarily conform to such sequence. Hence a confusion of ideas, which is often attributed to the stupidity of the child, but which is in reality due to the inexperience of the instructor; as, for instance, in the case exemplified by Pip, in Dickens' 'Great Expectations,' who, having imbibed the precept that he was to walk in the same all the days of his life, was led by his sequence of ideas to infer therefrom that he was invariably to walk to school by the same path, and on no account go round by the pastrycook's.

And so in studying savages and early races whose mental development corresponds in some degree to that of children, we have to guard against this automorphism, as Mr. Spencer terms it, that is to say, the tendency to estimate the capacity of others by our own, which appears almost completely to incapacitate some people from dealing with the subject.

The question of the free will of man enters largely into this study. I shall not be expected to say much upon a subject which has so lately occupied the attention of the public, having been discussed by some of our ablest scientists; but I cannot avoid quoting, in reference to this point, a passage from Dr. Carpenter's 'Mental Physiology,' who in this controversy is certainly entitled to be regarded as the champion of free will; and therefore by quoting him we run no risk of overstating the case against free will. "Our mental activity," he says, "is entirely spontaneous or automatic, being determined by our congenital nervous organism. It may be stated as a fundamental principle that the will can never originate any form of mental activity. . . . But it has the power," he continues, "of selecting any one out of several objects that present themselves either simultaneously or successively before the mental vision, and of so limiting and intensifying the impression which that particular object makes upon the consciousness, that all others shall be for the time non-existent to it."

The truth of this, in so far as regards the limitation of the will, cannot fail to force itself upon the student of culture. It is, I venture to think, by classifying and arranging in evolutionary order the actual facts of the manifestations of mind, as seen in the development of the arts, institutions, and languages of mankind, no less than by comparative anatomy, and far more than by metaphysical speculation, that we shall arrive at a solution of the question, to what extent the mental Ego has been, to use Professor Huxley's expression, a conscious spectator of what has passed.

I propose, therefore, with your permission, to give a few examples, by means of diagrams, of material evolution derived from the earliest phases of culture. In language and in all ideas communicated by word of mouth there is a hiatus between the limits of our knowledge and the origin of culture which can never be bridged over, but we may hold in our hand the first tool ever created by the hand of man.

It has been said that the use of speech is the distinctive quality of man. But how can we know that? We are literally surrounded by brute language. We can imitate their calls, and we find that animals will respond to our imitations of them. But who has ever seen any of the lower animals construct a tool and use it.

The conception of man, not as a tool-*using* but as a tool-*making* animal, is clear, defined, and unassailable; probably if we could trace language to its sources, we should be able to draw the same line between natural sounds employed as a medium of communication, and the created word. Thus the arts which we can study may perhaps be taken to illustrate the origin of language, which we cannot study in this phase.

The ape employs both sticks and stones as missiles and as hammers to crack the shells of nuts. But we have no evidence that he even selects special forms for special uses. The arts therefore afford us a clearly defined starting point for the commencement of culture.

To go in search of a particular form of stick or stone in order to apply it to a particular use would require greater effort of the will in fixing attention continuously on the matter in hand, than is found to exist amongst the lower animals except in cases of instinct, which term I understand to mean an inherited congenital nervous organism which adapts the mind to the ready reception of experience of a particular kind. But this instinct does not exist in the case in question; there is no tool-making instinct: our tool has to be evolved through reason and experience, without the aid of any special organism for the purpose.

The process we have to assume therefore is that, in using stones as hammers, they would occasionally split. In using certain stratified rocks this would occur frequently, and so force itself on the attention of the creature. The creature going on hammering, it would force itself on his notice that the sharp fractured end was doing better work than before. It would be perceived that there were hard things and soft things, that the hard things split the stone, and the soft things were cut by it; and so there would grow up in the mind an association of ideas between striking hard things and splitting, and striking soft things and cutting, and also a sequence by which it would be perceived that the fracture of the stone was a necessary preliminary to the other; and in the course of many generations, during which the internal organism of the mind grew in harmony with this experience, the creature would be led to perform the motions which had been found effectual in splitting the stone before applying it to the purposes for which it was to be used.

Thus we arrive at a state of the arts in which we may suppose man to be able to construct a tool by means of a single blow. By constantly striking in the same direction, flakes would be produced; and by still further repeating the same motions, it would at last be found that by means of many blows a stone could be chipped to an edge or a point so as to form a very efficient tool.

But this continued chipping of the stone in order to produce a tool, implies a considerable mental advance upon the effort of mind necessary to construct a tool with one blow.

It implies continued attention directed by the will to the accomplishment of an object already conceived in the mind, and its subsequent application to another object which must also have been conceived in the mind before the tool was begun.

Now we know from all experience, and from all evolution which we can trace with certainty, that progress moves on in an accelerating ratio, and that the earlier processes take longer than the later ones.

But the implements of the drift, which are the earliest relics of human workmanship as yet recognized, are most of them multiflaked tools, such as the implement which I hold in my hand, requiring a considerable time to construct it, and the use of innumerable blows in order to trim it to a point at one end.

It appears therefore evident that in the natural course of events the drift period must have been preceded by an earlier period of considerable extent characterized by the use of single-flaked tools. And we may therefore consider it probable that should any evidence of man be hereafter discovered in miocene beds, they will be associated with such large rude flakes as those now exhibited, which require a feebler effort of attention and of reason to construct.

If we examine the forms of the flint implements of the drift, we find that out of many intermediate shapes we may recognize three in particular, which have been minutely described by Mr. Evans in his valuable work on the stone implements of Britain; 1st, a side tool, consisting of a flint chipped to an edge on one side and having the natural rounded outside of the flint left on the other side, where it appears to have been held in the hand; 2nd, a tongue-shaped implement chipped to a point at one end, and having the rounded surface for the hand at the big end; and 3rd, an oval or almond-shaped tool, which is often chipped to an edge all round.

We have no evidence to show which of these kind of tools was the earliest; but that they were employed for different uses there can be little reason to doubt. But have we any evidence to throw light on the way in which these several forms originated in the minds of men in the very low condition of mental development which we may suppose to have existed at the time?

About eight years ago, whilst examining the ancient British camps on the South Downs, I chanced to discover in the camp of Cissbury, near Worthing, a large flint factory of the neolithic age. There were some sixty or more pits from which flints had been obtained from the chalk, and these pits were full of the débris of the flint workers. The factory was of the neolithic age, the most characteristic tool of which is the flint celt, a form which differs but slightly from the oval or almond-shaped palæolithic form, but the cutting edge of which is more decidedly at the broad end. The débris, some six hundred or more specimens of which were collected, consisted chiefly of these celts in various stages of manufacture.

If anyone will attempt to make a flint celt, as I have done sometimes (and Mr. Evans, from whom I learnt that art, has done frequently), he will find that it is difficult to command the fracture of the flint with certainty; every now and then a large piece will come off, or a flaw will be discovered which spoils the symmetry of the tool, and it has to be thrown away. In arranging and classifying the remains of this flint factory, I found that all the palæolithic forms were represented by one or other of these unfinished celts, so much so as to make it doubtful whether some of them may not actually have been used like them.

A celt finished at the thin end, and abandoned before the cutting edge was completed, represented a tongue-shaped palæolithic implement; a celt finished only on one side represented a palæolithic side tool; and a celt rudely chipped out, and abandoned before receiving its finishing strokes, represented almost exactly an oval palæolithic tool, only differing from it in being somewhat rougher, and showing evidence of unfinish.

Taking a lesson then from this flint-worker's shop of the later neolithic age, we see how the earlier palæolithic forms originated. They were not designed outright, as the nineteenth-century man would have designed them for special uses, but arose from a selection of varieties produced accidentally in the process of manufacture. The forms were also suggested by those of the nodules out of which they were made. We see, by examining the outside surfaces that were left on some of them, how a long thin nodule produced a long thin celt, a broad thick nodule a broad thick celt, and so forth. Indeed, so completely does the fabricator appear to have been controlled by the necessities of his art, that in tracing these successive forms one is almost tempted to ask whether the principle of causation lay mostly in the flint or the flint worker, so fully do they bear out the statement of Dr. Carpenter and the other physiologists, that nothing originates in the free will of man.

On these two diagrams (Plates I. and II.) I have shown how from the same form of palæolithic implement already described, the more complex forms of the spear and axe blade of the subsequent periods were developed. The point developed into a spear, and the broad end into an axe blade. You will see by reference to Plate I. that the oval tool of the drift suggested the smaller leaf-shaped spear-head of the early neolithic age. This, by a gradual straightening of the sides, became the lozenge-shaped form, which latter developed into the barbed form, and this last into the triangular form, which consists of barbs without a tang.

On the other hand, this same oval tool of the drift (Plate II.), when used as an axe blade with the broad end, became the celt of the neolithic period, chipped only at first and subsequently polished. This gave rise to the copper celt of the same form having convex surfaces, which grew into the bronze celt with flat sides. Then the bronze celt was furnished with a stop to prevent its being pressed too far into the handle by the blow. Others were furnished with projecting flanges to prevent them from swerving by the blow when hafted on a bent stick. Others had both stops and flanges. By degrees the flanges were bent over the stops and over the handle, and then the central portion above the stops, being no longer required, became thinner, and ultimately disappeared, the flanges closed on each other, and by this means the weapon grew into the socket celt. On this socket celt you will see that there is sometimes a semicircular ornamentation on each side. This semicircular ornament, as I pointed out, in a paper on primitive warfare read some time ago, is a vestige of the overlapping flange of the earlier forms out of which it grew, which, like the rings on our brass cannon, are survivals of parts formerly serving for special uses.

In the vertical columns I have given in the order of their occurrence the successive periods of prehistoric time, viz. the early palæolithic, late palæolithic, early neolithic, late neolithic, early bronze, late bronze and iron periods, beneath which I have placed lines for two distinct phases of modern savage culture, viz. the Australian and the American Indian. A cross beneath each form denotes the periods in which they occur, and a vertical bar denotes that they are of rare or doubtful occurrence; so that the sequence of development may be seen at a glance, and it is only a glance that I ask you to take at these diagrams on the present occasion. I have checked them with Mr. Evans' work and also with Sir William Wilde's Catalogue, and I do not think that any of the statements made in them will be challenged; but as these forms were not developed for the purpose of filling in the spaces in rectangular diagrams, such diagrams only imperfectly convey an idea of the evolution which has taken place, and must be regarded only as provisional and liable to be improved.

In tracing the evolution of prehistoric implements, we are of course limited to such as were constructed of imperishable materials. No doubt our prehistoric ancestors used also implements of wood, but they have long since disappeared; and if we wish to form an idea of what they were, we must turn to those of his nearest congener, the modern savage.

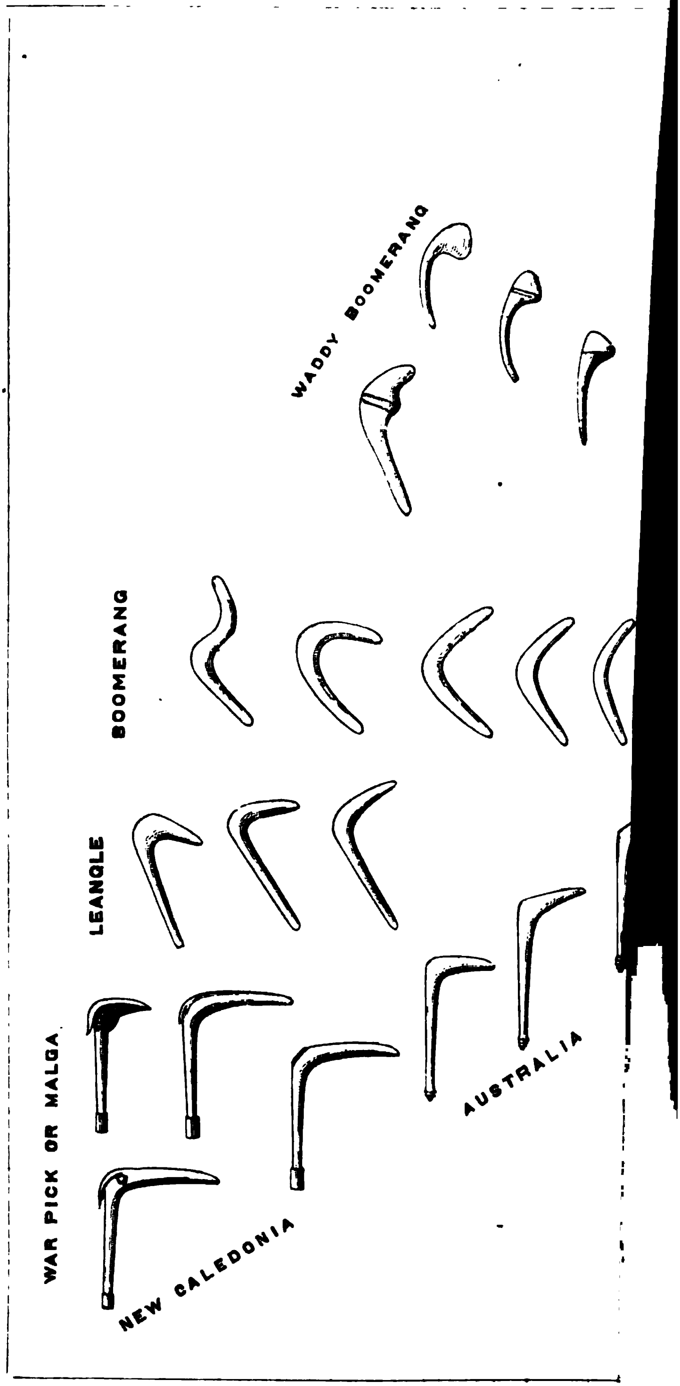
In speaking of savages, the question of progression *versus* degeneration is probably familiar to most of those present, through the writings of Sir John Lubbock and Mr. E. B. Tylor. To the several weighty arguments in favour of progression given by those writers I will add this one derived from the sequence of ideas.

If the Australians, for example, were the degenerate descendants of people in a higher phase of culture, then, as all existing ideas are made up of previous ideas, we must inevitably find amongst their arts traces of the forms of earlier and higher arts, as is the case amongst some of the savages of South America who early came in contact with Peruvian civilization; but the reverse of this is the case; all the forms of the Australian weapons are derived from those of nature.

In the same way that we saw that the forms of the palæolithic flint implements were suggested by accidental fractures in the workshop, so the several forms of the Australian wooden implements were suggested by the various forms of the stems and branches out of which they were made. These savages, having only flint tools to work with, cannot saw out their weapons to any form they please; they can only trim the sticks into a serviceable shape. All their weapons are therefore constructed on the grain of the wood, and their forms and uses have arisen from a selection of the natural curves of the sticks.

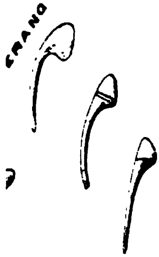
I have arranged on Plate III. drawings of nearly all the weapons used by the Australians, placing them together according to their affinities in such a manner as to show hypothetically their derivation from a single form. As all the forms given on this diagram are drawings of weapons in use at the present time, and there are many inter-

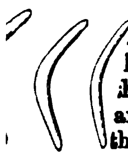
Plate III,


WAR PICK OR MALGA
NEW CALEDONIA
AUSTRALIA
LEANGLE
BOOMERANG
WADDY BOOMERANG

d wrong from here.

ıot given here, I have not arranged them in horizontal ı previous diagrams, to show their place in time, but them as radiating from a central point. We know e antiquities of savage countries as yet, and therefore heir evolution in time. The development has therefore y means of survivals of early forms existing at the present





ıntre I have placed the simple cylindrical stick, as being t form. By a gradual development of one end I have ırds the formation of a sharp ridge and its transition into ıushroom form. To the right upwards I have traced the opment of the mushroom head, the projecting ridge of which tly liable to fractures by blows; and as savages always e accidental fractures so as to produce symmetry, scollops cut out of the ridge in different places for this purpose, l the effect of concentrating the force of the blow on the ıs. These were further developed; one of the pilei of the m head was made larger than the others, and this suggested . of a bird's head, so that it was only necessary to add a line nouth and a couple of eyes to complete the resemblance. To ıt we see that the plain stick held in the centre gave the first a defensive weapon, and was used to parry off the darts of the nt; an aperture was then made in the stick for the hand, and ce of it became broader, developing into a shield, the narrow however, being still retained for parrying. Below I have shown he long stick simply pointed at one end became a lance; a row arp flints were gummed on to one side to produce a cutting edge, these were then imitated in wood, and by pointing them obliquely were converted into barbs. To the right another kind of barb produced by binding on a piece of sharp-pointed wood. Between and the shields we see that the first idea of the throwing-stick, ployed to project these lances, was simply constructed like the rbed point of the lance itself. The gradual expansion of the stick ose from its being employed like a battledore, to fence off the enemy's nces. To the left below I have shown the gradual development of peculiar curved weapon, called the malga, formed from a stem, nd the branch projecting from it at different angles. The part where the continuation of the stem was cut off was trimmed to a kind of ridge; this ridge developed, and suggested the crest of a bird's head; ultimately the eyes were added, in the same manner as in the club on the opposite side of the diagram. To the left we see the plain round stick first flattened, then curved. Savages are in the habit of throwing all their weapons at their adversaries and at animals. In throwing a flat curved stick it rotates of its own accord, and as the axis of rotation continues parallel to itself, the thin edge is presented to the resistance of the air in front; this increases the range, and its peculiar flight must have forced itself on the attention of the savage as the result of experience: but he has never had the slightest knowledge of the laws of its flight. The different curves of the boomerang are the natural curves of the sticks, and like all the Australian weapons, they are made on the grain of the wood. Some are thicker than others; some will fly in the curves peculiar to that weapon, and others will not: scarcely two are alike.

To the left above, we see the mushroom-headed waddy, with its projecting ridge flattened, then curved; one side becomes more developed than the other, and this being thrown develops into the waddy boomerang, the ridge of the earlier forms being still represented by a mark on the flat head of the weapon; an intermediate link connects it with the true boomerang.

Many other examples might be given to illustrate the continuity which exists in the development of all savage weapons; but I only ask you to glance at the sequence shown in this diagram and the preceding ones in order to convince you of the truth of the statement which I made at the commencement of this discourse, that although, owing to the complexity of modern contrivances and the larger steps by which we mount the ladder of progress in the material arts, their continuity may be lost sight of, when we come to classify the arts of savages and prehistoric men, the term growth is fully as applicable to them as to the development of the forms of speech, and that there are no grounds upon the score of continuity, history, or the action of free will to separate these studies generically as distinct classes of science.

But in dealing with evolution we have to speak not only of growth, but, as in all other natural sciences, of the principle of decay. By decay I do not mean the decay of the materials of the arts, but the decomposition of the mental ideas which produced them.

As complex ideas are built up of simple ones, so there is also a further process by which they become disintegrated, and the parts go to form parts of other ideas.

This decay in the arts corresponds to what is called phonetic decay in language; and in both cases it arises either from incapacity, the desire to save trouble, or the necessity of abbreviating when ideas originally evolved for one purpose come to form parts of other ideas to which they are merely accessory and subordinate, as in the well-known dialectic changes of speech. Every sound in language had originally a distinct meaning of its own; gradually these sounds or roots came to form parts of words in which the original meanings of the sounds were lost.

I will now endeavour to draw a parallel to this in the arts, by means of what may be termed realistic degeneration.

I will not say much as to the place of realism in culture. The archæological world has lately been somewhat startled by the discovery of well-executed designs of elephants and other animals in the French caves in association with the rude stone implements of the palæolithic age, and by the more recent discovery of Mariette-Bey, that the earliest Egyptian sculptures of the third dynasty are the most truthful representations of the human form that are to be found in that country. I see nothing surprising in this, when we consider the power that is developed in many children of eight or nine years old of making drawings of animals and other objects, which, when allowance is made for the feeble hand of childhood, are often as truthful as those of the cave-period men, at a time when their minds have acquired but little power of reasoning or generalizing, or even of taking care of themselves; all which goes to prove that this power of imitation, which is a very different thing from ideal art, is one of the most early developed faculties of the mind of man.

When the power of imitation had once been developed, it would naturally be made use of as a means of intercommunication; thus the drawing of a stag would be made to convey information to people at a distance that there was a herd of deer in the neighbourhood to be hunted; and as the object of the drawing was no longer to depict truthfully the peculiarities of the beast, but merely to convey information, the amount of labour expended upon it would be the least that could be employed for the required purpose. All written characters have originated in this way; and no one now requires to be told how pictographic representations developed into hieroglyphic and subsequently into phonetic characters.

But realistic degeneration would equally take place in all cases in which pictorial representations came to be employed for other purposes than those for which they were originally designed, as in the case of ornamental designs.

So also a coin receives upon its surface the image of a king or a god as a stamp of authority. When from any cause the object of the original design is lost, the object of the stamp being no longer to convey a likeness, but being merely used as a test of genuineness, or perhaps amongst an unlettered people to denote its value, the tendency to realistic degeneration would be proportioned to the difficulties of execution; no further labour would be expended on it than was necessary for the object to be attained. Here I must again remind you of the interesting discourse delivered in this Institution a fortnight ago, by Mr. Evans, on the evolution of British coins. With his permission I have introduced some of his diagrams. You will remember how the coin of Philip of Macedon having been introduced into Britain, the head on the obverse gradually disappeared, leaving only the wreath as a band across the coin, which was ultimately converted into a cross; and how on the reverse, the chariot and two horses dwindled into a single horse, the chariot disappeared, leaving only the wheels, the driver became elevated, not elevated after the manner unfortunately but too common amongst London drivers, but elevated after the manner of the Spiritualists, except that you see he had the precaution to take on a pair of wings, differing also both from the London driver and the Spiritualists, inasmuch as instead of having lost his head he has lost his body, and nothing but the head remains; the body of the horse then gradually disappears, leaving only four lines to denote the legs.

I will now show you an exact parallel to these transformations in a collection of designs, supposed to be tribal marks, which are drawn upon the paddle blades of the New Irelanders, a race of Papuan savages inhabiting an island on the north-east coast of New Guinea.

Having noticed one or two allied varieties of design in specimens that came into my possession, I determined to collect all that I could find as they came to this country. In the course of several years I succeeded in obtaining the series represented upon Plate IV.

The first figure you will see clearly represents the head of a Papuan: the hair or wig is stuffed out, and the ears elongated by means of an ear ornament, after the manner of these people; the eyes are represented by two black dots, and the red line of the nose spreads over the forehead. This is the most realistic figure of the series. In the second figure the face is somewhat conventionalized: the line of the nose passes in a coil round the eyes; there is a lozenge pattern on the forehead, representing probably a tattoo mark; the body is represented sitting in full. In the third figure the man is represented sitting sideways, simply by lopping off an arm and a leg on one side. In the fourth figure the legs have disappeared. In the fifth figure the whole body has disappeared. In the sixth figure the nose has expanded at the base, and the sides of the face are made to conform to the line of the nose; the elongated ears are there, but the ear ornament is gone: the nose in this figure is becoming the principal feature. In the seventh figure nothing but the nose is left: the sides of the face and mouth are gone; the ears are drawn along the side of the nose; the head is gone, but the lozenge pattern on the forehead still remains; the coil round the eyes has also disappeared, and is replaced by a kind of leaf form, suggested by the upper lobe of the ear in the previous figures; the eyes are brought down into the nose. In the eighth figure the ears are drawn at right angles to the nose. In the ninth figure the nose has expanded at the base; all the rest is the same as in the last figure. In the tenth figure the lozenge pattern and the ears have disappeared, and a vestige of them only remains, in the form of five points; the base of the nose is still further expanded into a half moon. In the last figure, nothing but a half moon remains. No one who compared this figure with the first of the series, without the explanation afforded by the intermediate links, would believe that it represented the nose of a human face. Unfortunately we do not know as yet the exact meaning of these designs, but when further information is obtained about them it will throw considerable light on similar transformations in prehistoric times.

My next and last illustration is taken from the relics of Troy, recently brought to light by Dr. Schliemann.* In the valuable work lately published by him he gives illustrations of a number of earthen-

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* For illustrations, see 'Troy and its Remains,' by Dr. Henry Schliemann. Murray, 1875. The figures may be taken in the following order: No. 185, No. 74, No. 132, No. 13, No. 173, No. 207, No. 12, No. 11, No. 133, No. 141, No. 165.

Plate IV.

Fig. 1.

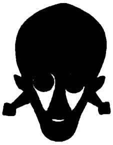


Fig. 2.

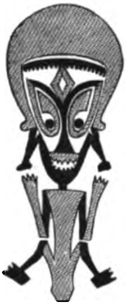


Fig. 3.

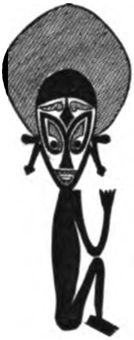


Fig. 4.

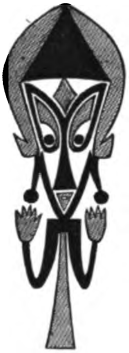


Fig. 5.

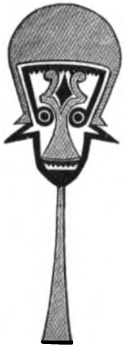


Fig. 6.

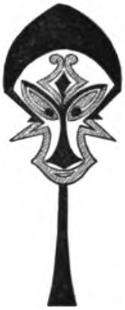


Fig. 7.

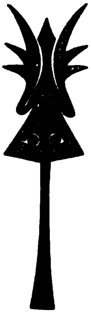


Fig. 8.

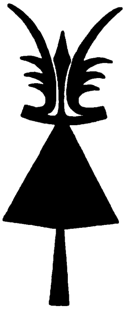


Fig. 9.

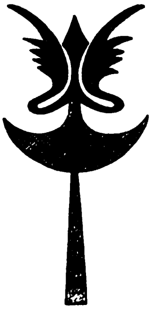


Fig. 10.

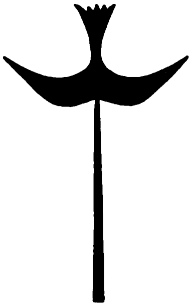
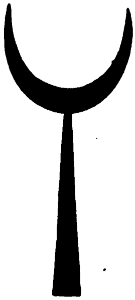


Fig. 11.



ORNAMENTATION OF NEW IRELAND PADDLES, SHOWING THE TRANSITION OF FORM.

ware vases and other objects called by him idols, having on them the representation of what he conceives to be the face of an owl, and which he believes to represent Athena, the tutelary goddess of Troy, called by Homer Glaukopis Athene, which signifies, according to him, "with the face of an owl." Professor Max Müller has given his opinion that the word "glaukopis" cannot possibly be taken to mean owl-faced, but can only mean large or bright-eyed. On this point I will venture no opinion, but accepting Professor Müller's high authority for the usually received interpretation of it being correct, I shall in no way weaken the evidence in favour of Dr. Schliemann's discovery of the true site of Troy if I succeed in proving that, according to the true principle of realistic degeneration, this figure does not represent an owl but a human face.

The figures on this diagram are all taken from Dr. Schliemann's representations, and as the depth of each is given it will be seen that the different varieties of face occur in all the different strata excavated by him except the highest, and therefore no argument as to antiquity can be based upon the depth at which they were found. The two first figures it will be seen are clearly intended to represent a human face, all the features being preserved. In the two next figures the mouth has disappeared, but the fact of the principal feature being still a nose and not a beak, is shown by the breadth of the base and also by the representation of the breasts. In the two succeeding figures the nose is narrowed at the base, which gives it the appearance of a beak, but the fact of its being still a human form is still shown by the breasts. Had the idea of an owl been developed through realistic degeneration in these last figures, it would have retained this form, but in the two succeeding figures it will be seen that the nose goes on diminishing.

In the five next rows of figures, which are of solid stone, not earthenware, and which Dr. Schliemann believes to be gods, it is clearly shown by the rude scratches representing the eyebrows, and their want of symmetry, that this degeneration of form is the result of haste.

What then are these solid stone objects? I cannot for a moment doubt, from their resemblance to the vases, from the marks denoting the junction of the cover with the vase, and from the representations of handles, that they are votive urns of some kind, similar to those Egyptian stone models of urns represented in the two figures above. Urns of this kind were used by the Egyptians to contain the viscera of the mummies; but with the cheaper form of burial, in which the viscera were retained in the body, stone models of urns, of which these figures are drawings from originals in the British Museum, were deposited in the graves as vestiges of the earlier and more expensive process; these objects therefore cannot be idols, but votive urns. The fact of human remains having been found in some of the human-headed urns, and the hasty scratches on the stone models, show that they are merely models appertaining to the conventionalized survival of some earlier or more elaborate system of urn burial.

We see from these facts that both growth and decay, the two component elements of evolution, are represented in the study of the material arts.

My object in this discourse has been not, as I fear it may have appeared to you from the brief time at my disposal and my imperfect treatment of the subject, to extol the material arts as being intrinsically of more interest or importance than other branches of culture, but to affirm the principle that it is by studying the psychology of the material arts alone that we can trace human culture to its germs.

The theory of degradation is supported only by the study of those branches of culture of which the early history is lost.

The tree is the type of all evolution: all trees are seedlings, but they differ in their mode of growth. Some, like the beech and oak, throw their branches upwards, and these are typical of the development of the material arts; others, like the straight-stemmed pine, throw off their branches downwards, and these are typical of the development of some other branches of culture. It is quite true, as stated by mythologists, that the history of myths is one of continued degeneration in so far as they can be traced, and that the element of decay enters far more into their composition than that of growth. But the whole accessible history of these myths represent drooping branches from the upward growing stem of free thought out of which they sprang. What is the space of time which separates us from the Vedas as compared with the whole upward growth of humanity before and since!

There are huge gaps in our knowledge of the history of the human race, and it has been the pleasure of mankind in all ages to people these gaps with jugglers and bogies; but surely, if slowly, science will open up these desert places, and prove to us that, so far as the finite mind of man can reach, there is nothing but unbroken continuity to be seen in the present and in the past.

[A. L. F.]

# NOTES OF PROFESSOR GLADSTONE'S FIFTH LECTURE ON CHEMICAL FORCE.

Tuesday, June 1, 1875.

## *The Copper-Zinc Couple and its Effects.*

In an ordinary Voltaic cell the liquid is decomposed under the influence of the two different metals in contact, but this decomposition is always impeded more or less by the "resistance" of the liquid; indeed, in the case of very many compounds, especially organic liquids, this internal resistance is so great that practically no Voltaic decomposition is capable of being effected. When the junction of the metals is made outside the cell as in all batteries, there is also more or less external resistance. Now the resistance offered by a badly conducting liquid, such as pure water, depends upon the amount of it that lies between the two metals, and the power is most advantageously applied when the distance is reduced to a minimum, and there is no part of the circuit outside the liquid. This is effected in the copper-zinc couple, which has been recently applied by Professor Gladstone and Mr. A. Tribe in the laboratory of the Institution, and elsewhere, to the decomposition of many compound bodies, and the preparation of several substances hitherto unknown.

If a piece of zinc be immersed in a solution of cupric sulphate, copper is deposited upon it in a minute state of division, and thus the two metals touch at myriads of points. When this "couple" is immersed in a binary liquid, the liquid at each point of junction is exposed to the full chemical or electromotive force of the metals.

In this way pure water may be decomposed with the production of pure hydrogen gas; organic haloid bodies, such as ethylic iodide, and its congeners, may be split up, and thus there may be prepared zinc ethyl, zinc propyl, &c., bodies spontaneously inflammable in the air; the same haloid bodies in the presence of alcohol may be decomposed with the production of the hydrides of the radicals, such as marsh gas, and a new set of compounds the zinc haloid ethylates; chloroform, bromoform, and iodoform may be made to yield the same hydrides, haloid ethylates, and acetylene; while isomeric bodies such as the chlorides of ethylene, and ethylidene, may be examined as to their chemical structure.

The importance of time as an element in chemical action is well illustrated in many of these decompositions; and they are generally greatly facilitated by a rise of temperature, whether that result from the chemical action itself, or from external sources.

The copper-zinc couple has been practically applied to the production of certain organic compounds, and to the determination of nitrates in potable waters.

# WORK DONE BY MEANS OF GLADSTONE AND TRIBE'S COPPER-ZINC COUPLE.

| Date. | Chief Results. | Chemistry of Operation. |
|---|---|---|
| 1872 | Decomposition of Water, and Preparation of Pure Hydrogen | $ZnCu+2H_2O=Cu+Zn2HO+H_2$ |
| 1873 | Direct Formation of Zinc Ethiodide, and | $ZnCu+C_2H_5I=Cu+Zn\left\{\begin{matrix}C_2H_5\\I\end{matrix}\right.$ |
| | Preparation of Zinc Ethyl, Ethyl | $2Zn\left\{\begin{matrix}C_2H_5\\I\end{matrix}\right.=Zn\left\{\begin{matrix}C_2H_5\\C_2H_5\end{matrix}\right.+ZnI_2$ |
| | Hydride and Zinc *Iodo-ethylate* | $ZnCu+\left\{\begin{matrix}C_2H_5O\\H\end{matrix}\right.+\left\{\begin{matrix}C_2H_5\\I\end{matrix}\right.=Cu+Zn\left\{\begin{matrix}C_2H_5O\\I\end{matrix}\right.+\left\{\begin{matrix}C_2H_5\\H\end{matrix}\right.$ |
| 1873 | Preparation of Di-amyl, | $ZnCu+2\left\{\begin{matrix}C_5H_{11}\\I\end{matrix}\right.=Cu+\left\{\begin{matrix}C_5H_{11}\\C_5H_{11}\end{matrix}\right.+ZnI_2$ |
| | Zinc-amyl, and Amyl hydride. | $2ZnCu+2\left\{\begin{matrix}C_5H_{11}\\I\end{matrix}\right.=Cu+Zn\left\{\begin{matrix}C_5H_{11}\\C_5H_{11}\end{matrix}\right.+ZnI_2$ |
| | | $ZnCu+\left\{\begin{matrix}C_2H_5O\\H\end{matrix}\right.+\left\{\begin{matrix}C_5H_{11}\\I\end{matrix}\right.=Cu+Zn\left\{\begin{matrix}C_2H_5O\\I\end{matrix}\right.+\left\{\begin{matrix}C_5H_{11}\\H\end{matrix}\right.$ |
| | Preparation of Methyl Hydride. | $ZnCu+C_2H_6O+CH_3I=Cu+Zn\left\{\begin{matrix}C_2H_5O\\I\end{matrix}\right.+CH_4$ |
| 1873 | Preparation of *Zinc Isopropyl*, | $2ZnCu+2\left\{\begin{matrix}C_3H_7\\I\end{matrix}\right.=Cu+Zn\left\{\begin{matrix}C_3H_7\\C_3H_7\end{matrix}\right.+ZnI_2$ |
| | *Zinc prop-iodide*, *Zinc propyl*, and Propyl Hydride | $ZnCu+\left\{\begin{matrix}C_3H_7\\I\end{matrix}\right.=Cu+Zn\left\{\begin{matrix}C_3H_7\\I\end{matrix}\right.$ |
| | | $2Zn\left\{\begin{matrix}C_3H_7\\I\end{matrix}\right.=ZnI_2+Zn\left\{\begin{matrix}C_3H_7\\C_3H_7\end{matrix}\right.$ |
| | | $ZnCu+\left\{\begin{matrix}C_3H_7\\I\end{matrix}\right.+\left\{\begin{matrix}C_2H_5O\\H\end{matrix}\right.=Cu+Zn\left\{\begin{matrix}C_2H_5O\\I\end{matrix}\right.+\left\{\begin{matrix}C_3H_7\\H\end{matrix}\right.$ |
| 1874 | Isolation of Di-allyl, and | $ZnCu+2\left\{\begin{matrix}C_3H_5\\I\end{matrix}\right.=Cu+\left\{\begin{matrix}C_3H_5\\C_3H_5\end{matrix}\right.+ZnI_2$ |
| | Preparation of Pure Propylene. | $ZnCu+\left\{\begin{matrix}C_3H_5\\I\end{matrix}\right.+\left\{\begin{matrix}C_2H_5O\\H\end{matrix}\right.=Cu+Zn\left\{\begin{matrix}C_2H_5O\\I\end{matrix}\right.+C_3H_6$ |
| 1874 | Preparation of Pure Olefiant Gas and its homologues | $ZnCu+\left\{\begin{matrix}C_nH_{2n}\\Br_2\end{matrix}\right.=Cu+ZnBr_2+C_nH_{2n}$ |
| 1874 | Preparation of *Zinc Ethylobromide*, and | $ZnCu+\left\{\begin{matrix}C_2H_5\\Br\end{matrix}\right.=Cu+Zn\left\{\begin{matrix}C_2H_5\\Br\end{matrix}\right.$ |
| | *Zinc Brom-ethylate* | $ZnCu+\left\{\begin{matrix}C_2H_5\\Br\end{matrix}\right.+\left\{\begin{matrix}C_2H_5O\\H\end{matrix}\right.=Cu+Zn\left\{\begin{matrix}C_2H_5O\\Br\end{matrix}\right.+\left\{\begin{matrix}C_2H_5\\H\end{matrix}\right.$ |

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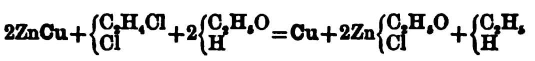
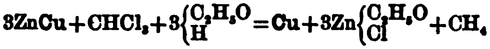
| Date. | Chief Results. | Chemistry of Operation. |
|---|---|---|
| 1874 | Preparation of *Zinc Chlorethylate*, and direct Hydrogenization of Ethylidene .. .. |  $2ZnCu+\left\{\begin{matrix}C_2H_4Cl\\Cl\end{matrix}\right.+2\left\{\begin{matrix}C_2H_5O\\H\end{matrix}\right.=Cu+2Zn\left\{\begin{matrix}C_2H_5O\\Cl\end{matrix}\right.+\left\{\begin{matrix}C_2H_5\\H\end{matrix}\right.$ |
| 1875 | Complete Hydrogenization of Chloroform, and .. .. .. |  $3ZnCu+CHCl_3+3\left\{\begin{matrix}C_2H_5O\\H\end{matrix}\right.=Cu+3Zn\left\{\begin{matrix}C_2H_5O\\Cl\end{matrix}\right.+CH_4$ |
| | Its homologues and preparation of Acetylene .. .. | $3ZnCu+2CHI_3=Cu+3ZnI_2+C_2H_2$ |
| | (Investigated by Prof. Thorpe.) | |
| 1873 | Conversion of Nitrates into Ammonia .. .. .. .. | $4ZnCu+6H_2O+KNO_3=Cu+4ZnH_2O_2+KHO+NH_3$ |

The new substances discovered during these investigations are in italics.

Cu signifies simply the metal copper, and not an atomic proportion.

## WEEKLY EVENING MEETING,

Friday, June 4, 1875.

The Duke of Devonshire, K.G. F.R.S. &c. Vice-President, in the Chair.

Professor Tyndall, D.C.L. LL.D. F.R.S. &c.

### *On Whitworth's Planes, Standard Measures, and Guns.*

Born on the 21st December, 1803, Sir Joseph Whitworth, to whom we are indebted for the materials of this evening's discourse, was placed at the age of fourteen under the care of his uncle, a millowner in Derbyshire. He quitted the mill at eighteen; went to Manchester, and worked there for four years, under Crichton, Marsden and Walker, and other employers. At the age of twenty-two he went to London, lived there for eight years as a journeyman mechanic, working in succession in the establishments of Maudslay, Holtzapfel, Wright, and Clements.* In 1833 he returned to Manchester, rented a room with steam power, and wrote over his door "Joseph Whitworth, toolmaker from London." † The ground on which his works now stand in Manchester is worth nearly a quarter of a million sterling.

His aim from the first was mechanical veracity, and his earliest step towards the realization of this aim was the production of true plane surfaces. The most accurate planes, when he began, were obtained by first planing and then grinding the surfaces. They were never true. He abandoned grinding for scraping. Taking two surfaces, as accurate as the planing tool could make them, he thinly coated one of them with colouring matter, and rubbed the other over it. Were both surfaces true, the colouring matter would spread itself uniformly over the upper one. It never did so, but appeared in spots and patches. These marked the eminences, which, with an appropriate tool, he scraped away. In this way he gradually rendered the surfaces more and more coincident. But the coincidence of two surfaces would not prove them to be planes. If one were concave and the other convex, they might still coincide. This was got over by taking a third surface, and adjusting it to both the others. Were one of the latter concave and the other convex, the third plane could not coincide with both. By a series of interactions and adjustments all three surfaces were at length rendered coincident; then, and not before, were they considered "true planes."

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* The extraordinary mechanical genius who was associated with Mr. Babbage in the construction of the calculating machine.

† Whitworth was the first to take out a patent for the making of tools.

When one true plane is placed upon another, the former *floats* upon the latter, as if some lubricating material existed between them. But when one of them is slidden, with pressure, over the other, they cling firmly together. The effect is thus described and accounted for by Sir Joseph Whitworth: "If one of them [the planes] be carefully slided over the other to exclude the air, the two plates will adhere together with considerable force, by the pressure of the atmosphere." * This clinging together of flat surfaces had been noticed before the time of Robert Boyle, and that great experimenter, and noble philosopher, first gave currency to the explanation adopted by Sir Joseph Whitworth. He experimented with slabs of marble, and with plates of glass, and thought he had proved that the clinging together did not occur in vacuo.† This explanation was experimentally tested in the discourse. Two exceedingly accurate hexagonal planes remained adherent in the best vacuum obtainable by a good air-pump. The vacuum was still further improved by filling the receiver with carbonic acid, and absorbing the residue with caustic potash. In this way the atmosphere was reduced until its total pressure on the surface of the hexagon amounted to only half a pound. The lower plate weighed 3 lbs., and to it was attached a mass of lead weighing 12 lbs. Though the pull of gravity was here thirty times the pressure of the atmosphere, the weight was supported. Indeed, it was obvious, when an attempt was made to pull the plates asunder, that had a weight of 100 lbs., instead of 12 lbs., been attached to the lower hexagon, it also would have been sustained by the powerful attraction of the two surfaces.

To show the probable character of the contact between the planes, two very perfect surfaces of glass were squeezed together with sliding pressure. They clung, apparently as firmly as the Whitworth planes. Throwing by reflexion from the glass plates a strong beam of light upon a white screen, the colours of "thin plates" were vividly revealed. Clasping the plates of glass by callipers, and squeezing them, the colours passed through various changes. When monochromatic light was employed the successions of light and darkness were numerous and varied, producing patterns of great beauty.* All this proves that, though in such close mechanical contact, the plates were by no means in optical contact, being separated by distances capable of embracing several wave-lengths of the monochromatic light.

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* Paper read before the British Association at Glasgow, 1840.

† Boyle's controversy with Hobbes, in relation to this subject, is highly interesting, as revealing the characters of the two men. "I never," says Boyle, "envied Mr. Hobbes's forwardness to triumph, and am content his conjectures be recommended by the confidence which accompanies them, if mine be by the success which follows them."—Boyle's 'Works,' vol. iv. p. 111.

Having obtained his true planes, Whitworth turned them to account in producing standard measures. His paper on this subject is of profound practical interest. His aim is to spread abroad notions of attainable mechanical accuracy, far in advance of his time. He pitted the sense of touch against the sense of sight,—end-measurement, felt by the fingers, against line-measurement seen by the eye,—and succeeded in proving that the millionth part of an inch was capable of accurate measurement. With instruments capable of this accuracy he produced his standard screws and "difference gauges," urging impressively upon the mechanicians of his age the enormous waste which might be avoided by the adoption of uniform dimensions, capable, when accurately measured, of being accurately reproduced.

The mechanical genius of Mr. Whitworth was next applied to the improvement of the rifle. But a few preliminary words on the use of rifling may be introduced here. When a lead bullet is cut in two, a cavity is always found within, produced by the contraction of the metal. This, and other causes, render coincidence between the centre of gravity and the geometric centre of the ball the exception, and not the rule; and where this coincidence does not exist the bullet is almost sure to quit the gun with a motion of rotation added to its motion of translation. It is then found to deviate, sometimes to the right, sometimes to the left of the true trajectory. Sometimes, moreover, it exceeds, and sometimes falls short of the proper range. The cause of this deviation was first assigned by a philosopher who made the world his debtor in two ways: firstly, by his own important original contributions to science; and secondly, by the liberal aid and encouragement which he was always ready to extend to the younger scientific worker. I allude to the late Professor Magnus of Berlin.

Magnus's explanation of the deviation of projectiles is by no means difficult to grasp. That the deviation was produced by no shock of the projectile against the muzzle of the gun was proved by the fact that the deviation augmented in a quicker ratio than the distance from the gun. The cause of the deviation must be sought in the external air. If we blow through a tube so as to cause a current of air to pass close to the flame of a candle, the flame, instead of retreating, bends towards the current. The atmospheric pressure, in fact, is less in the current than elsewhere; hence arises a motion of the adjacent air towards the current. If a sheet of foolscap be held with its two leaves enclosing an angle of 20 or 30 degrees, on blowing through a tube between the leaves they close up, instead of flying asunder. The old experiment of Clement and Desormes is thus explained: a tube is fixed in the centre of a perforated disk, and a second unperforated free disk is brought up against the first. On blowing through the tube the second disk is not driven away, but on the contrary held fast until the current ceases, when it falls. In this case we have a radial spreading out of the air when it strikes the free disk, and in this radial current the atmospheric pressure is so much diminished that the excess of the external pressure supports the disk.*

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* On warming one of the plates by the breath, the travelling of the fringes was rendered beautifully manifest. This was the late Professor Baden Powell's mode of illustrating the repulsive power of heat.

We have now to apply the knowledge thus gained to the deviation of projectiles. Let us suppose ourselves looking down on the bullet as it quits the gun, that the axis of rotation is vertical and perpendicular to the trajectory, and that the direction of rotation is right-handed, like that of the hands of a watch. So circumstanced, the bullet will infallibly deviate to the right of the true trajectory. If, on the contrary, the rotation be left-handed, the deviation will be to the left. In one case only can the ball rotate without having this deflecting force acting upon it, and that is when the axis of rotation is a tangent to the trajectory. *It is the object of rifling to impress upon the projectile, at the outset, this particular kind of rotation.*

But whence the deflecting force, when the axis of rotation is perpendicular to the trajectory? The ball cannot rotate without producing all round it a cyclone, by its friction against the air. On opposite sides of the ball the air of this cyclone moves in opposite directions. When such a ball passes rapidly through the air it encounters an opposing wind; and it is obvious that on one side of the trajectory this wind coincides with that produced by the rotation of the ball, while on the other side the two winds are opposed to each other, and more or less neutralize each other. The ball will move towards the side on which the strongest current exists, the pressure on that side being a minimum. When the axis of rotation is perpendicular to the trajectory, and horizontal, it is obvious that the proper range will be exceeded when the two currents are in opposition on the under surface of the ball, a lifting of the ball being the consequence; while the range will be too short when the currents coincide on the under surface, a depression of the ball being the consequence. Thus, by the coalescence of the wind of rotation, with the wind of translation, the deviation of the ball from its true trajectory is in all cases completely accounted for.

Prior to the year 1853 rifles had been made by hand labour. A Government Committee was then appointed to determine whether they could not be produced by machinery. The committee, after due inquiry, invoked the aid of Mr. Whitworth; but he, lacking the special experience, and unwilling to modify his works to the required extent, declined the offer. But the subject remained open, and, on condition that a gallery should be built, where experiments as to the best form of rifle might be conducted, Mr. Whitworth subsequently proposed to devise and construct the machinery necessary for the production of the *rifle barrel.* Lord Hardinge, then Commander of the Forces, obtained the assent of the Government to the proposed condition, and a gallery 500 yards long was erected on Mr. Whitworth's grounds near Manchester.

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* In the discourse, one disk was suspended above the other, the lower one being fixed and the upper one connected with an index. On blowing through the fixed disk the upper one was drawn down, the index moving through a foot or more. The same experiment was made with water instead of air.

At the time now referred to the Enfield rifle was considered to be very near perfection. This Lord Hardinge pointed out to Mr. Whitworth, at the same time stating that any improvement which would induce the Government to change the Enfield for another rifle, must be "*very decided.*"

The powder then employed for small arms consisted of the siftings of cannon powder. The first step of Whitworth was to demonstrate its inefficiency. It was afterwards abandoned in favour of a quicker burning powder. A spiral groove within the barrel constituted the rifling of the Enfield weapon. The ball was elongated, being a cylinder with a conoidal front. Its length was 1·81 diameters of the bore. A wooden plug was inserted into the ball behind, which, pressed by the gunpowder, forced the lead into the grooves, and secured the desired rotation. The pitch of the spiral was such that to make a complete rotation the ball would have to pass through a length of 78 inches. Whitworth soon abandoned the rifling by grooves, and fell back upon polygonal rifling. He formed two accurately-fitting semi-cylinders, and having made the inner surface octagonal, with a certain amount of twist, he placed the semi-cylinders together, surrounded them by steel rings, and thus built up his first rifle barrel. *It was* only 10 inches long, but it beat the Enfield rifle. He lengthened the Enfield bullet, but found that fired from the Enfield barrel it tumbled over. He then passed from a twist of 1 in 78 to 1 in 30, then considered excessive, and with the augmented twist he was able to employ a longer bullet. With a thoroughness worthy of all admiration he passed in succession to obliquities of 1 in 20, 1 in 10, 1 in 5, even to 1 in 1. He thus exhausted the subject, and decided finally in favour of the hexagonal barrel, with a twist of 1 in 20. The length of the bullet was three diameters of the bore.

The performance of the new weapon astonished everybody. The School of Musketry at Hythe was then under the direction of Colonel Hay, who, in a report to Lord Hardinge, thus speaks of the Whitworth rifle: "The shooting with the gun on Mr. Whitworth's principle is truly wonderful. . . . The mean absolute deviation of the shots fired would not, if calculated, be above 0·65 of a foot at 500 yards; whereas, at the same distance, the mean absolute deviation of the best target of 20 shots I can produce, exceeds 2 feet." The tabulated results show an enormous superiority of the Whitworth over the Enfield rifle. "In accuracy of fire," says the 'Times' of April 23, 1857, "in penetration, and in range, the rival of the Enfield exceeds it to a degree which hardly leaves room for comparison." The best performance previously noticed had been a deviation of 24 inches in 500 yards. The Whitworth deviation at 800 yards was barely half this amount, while the deviation of the Enfield at 800 was more than twice its deviation at 500. At 1400 yards the Whitworth deviation was 4·62 feet, while at this distance a target 14 feet square was not at all hit by the Enfield.

The lively interest manifested by Lord Hardinge in this inquiry was shared by Sir John Burgoyne. In a letter dated 11th December, 1857, he thus expresses his opinion of what has been achieved, and submits to Mr. Whitworth a new problem: "I am myself so fully satisfied with your musket as a *great* step in advance, and that I must expect to be early adopted, that I am anxious for the time when we can know more about its powers of penetration of different substances, say iron, steel, elm, oak, &c., or earth, with the hard bullet." Never, surely, was a challenge more thoroughly met. The Whitworth rifle sent its bullets through thirty-four half-inch elm boards; the Enfield penetrated twelve of the boards, and was stopped at the thirteenth. But a totally new capacity was here manifested. The form of the Whitworth rifling permits of the use of a steel bullet, and with such bullets, at the time here referred to, Mr. Whitworth pierced plates of iron half an inch thick, not only point blank, but at obliquities varying from 0° to over 50°. I am not aware that up to the present hour any other rifle has accomplished anything approaching to this.*

The conservative principle which resists all change, and which is so marked a feature of official life, may be a necessary element of greatness and strength to this country. It will, however, be admitted that resistance may be carried too far. The application of lightning-conductors to the fleet was officially resisted; and despite the foregoing results, which might well be regarded as conclusive, a committee of officers in 1859 reported to the Government that the bore of the Whitworth rifle "was too small for use as a military weapon." Augmented knowledge altered this opinion, and three years subsequently another committee reported "that the makers of every small-bore rifle, having any pretensions to special accuracy, have copied to the letter the three main elements of success adopted by Mr. Whitworth, viz. diameter of bore, degree of spiral, and large proportion of rifling surface." Finally, a special committee of officers in 1869 recommended the bore which had been recommended by Whitworth in 1857.*

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* Both the plates and the projectiles which pierced them were present during the discourse. It is worth while to consider the condition of the bullet fired from the rifle in which the twist was 1 to 1. Half only of the ordinary charge of powder could be employed: the full charge tore the bullet to pieces. But with the half charge the bullet penetrated 9 inches of elm. A thousand feet a second would be a moderate velocity to assign to the bullet. It would travel, therefore, through the last foot of the barrel in $\frac{1}{1000}$th of a second; but in this length it would accomplish twelve rotations; hence on quitting the gun its rate of rotation would be twelve thousand a second. The rotating mirror with which Wheatstone, Foucault, and Cornu made their memorable experiments on electricity and light, rotated eight hundred times in a second. It is, however, not improbable that the Whitworth bullet had a still more rapid rotation than that here assigned to it; that it accomplished a million rotations per minute.

Call now to mind that Mr. Whitworth in 1854 found rifling by grooves the accepted method. This he changed for polygonal rifling. He found the twist 1 in 78; this he changed to 1 in 20. He found the rifle bullet 1·81 diameters in length; and this he changed to 3 diameters. The Martini-Henry rifle, which is now the favourite weapon, is rifled polygonally, a heptagon being substituted for Whitworth's hexagon. The twist of this rifle is 1 in 22, or almost identical with the twist of the Whitworth rifle, while the length of the bullet is 2·93 diameters, which is practically Whitworth's proportion. The Martini-Henry bullet is, however, rifled within the gun by the pressure behind it; and to permit of the employment of hard alloys of tin and lead, which it would be difficult to force into the angles of the heptagon, a sharp spiral projection accompanies the polygonal twist. This projection is forced into the bullet by the pressure from behind, and it gives the bullet the required rotation. The Whitworth bullet, on the other hand, is rifled outside the gun. Its inventor accomplishes by his steam-engine the work which, in other rifles, falls upon the gunpowder; the brunt of this work, moreover, being borne by the shoulder of the marksman. The Martini-Henry rifle is unable to meet the condition deemed so important by Sir John Burgoyne, and to fulfil which steel bullets are necessary.

To sum up, when Sir Joseph Whitworth began his experiments, he was as ignorant of the rifle as Pasteur was of the microscope, when he began his immortal researches on spontaneous generation. But, like the illustrious Frenchman, Whitworth mastered his subject to an extent never previously approached. He found the powder used for rifles unfit for its purpose. In point of precision, he obtained a "figure of merit" greatly superior to any previously obtained. He carried his ranges far beyond all previous ranges; and in point of penetration achieved the unexampled results which have been laid before you. He did this, moreover, by a system of rifling peculiar to himself, which had never been thought of previously, and which is substantially adhered to in the favourite weapon of to-day. It would be difficult to point to an experimental investigation, conducted with greater sagacity, thoroughness, and skill, and which led to more important conclusions.

It is the province of the generalizing faculty to extend to the largest phenomena the principles discerned in the smallest. With a directness of insight which he might find it difficult to define in words,

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* 'Guns and Steel,' p. 24.

Whitworth saw that the mechanical principles brought to bear in the infantry weapon were equally applicable to cannon. But here let me say that I am not too much influenced by the phrase "mechanical principles," so frequently employed by our great mechanician. These principles are as enduring as the universe, or rather as the mind which interprets the universe, when all the data have been embraced in the conclusion. But in practical matters mechanical principles, like wisdom, must be justified of their children; and these are the tests of experiment. It is now my duty to state to you with the necessary brevity, and with such power of illustration as I can command, how the Whitworth cannon has borne the experimental tests to which it has been subjected.

Before you is a piece of armour plating 4 inches thick, manufactured by an eminent firm, with the knowledge that it was to be tested with the Whitworth gun. Beside it is an elongated, flat-headed, rifled projectile, hardly exhibiting the least mark of distortion. Yet the plate has been perforated by that steel bolt. The gun which fired the bolt was a 12-pounder; but owing to its elongation the actual weight of the projectile is 29 lbs. This assuredly was a great achievement for so small a gun. You have seen what the Whitworth rifle accomplished with steel bullets. Before you is a series of parallel results obtained with the Whitworth cannon. The plates here presented to you have been perforated, not only by direct impact, but at obliquities of 35°, 45°, and 65°. This, to an outsider like myself, appears to be a result of extreme importance; and the reason I say so will be immediately clear. The projectiles which did this work are flat-headed. But I have now to direct your attention to two other plates which have borne the impact of two ogival pointed shot, the one of "chilled" metal, the other of steel. Instead of being penetrated, the plates are indented merely. Fired at an angle of 45° these pointed shots glanced from the plates, scooping out a small hollow, instead of piercing them. In this experiment, the chilled shot broke up into fragments, but the steel one is intact. Taking a steel bolt in the hand, and urging the edge of its flat front obliquely against an iron plate, the bolt is arrested, because the edge cuts the plate like a chisel; urging the pointed shot at the same obliquity against the plate, it glances off. Thus, a mere hand-experiment shows the difference between the flat-headed and pointed shot, when the incidence is sufficiently oblique. And as in actual warfare oblique incidence will probably be the rule, and perpendicular incidence the exception, this demonstrated power of the flat-headed bolt to penetrate, when the pointed shot fails, seems to me worthy of the most serious consideration.

Almost equally instructive are the experiments executed to determine the comparative power of flat-headed, round-headed, and pointed projectiles to penetrate water at an oblique incidence. The angle of depression was 7° 7′, and the length of water to be penetrated was 80 inches, the mark aimed at being 10 inches below the water line. The flat-headed projectiles appear to have gone almost directly to the mark; the round-headed ones were tilted up and struck the plate just below the water line; while the pointed or ogival ones were completely ejected from the water, and struck the plate at 9 inches above the water line.* These are points regarding which, one would think, no uncertainty ought to exist; they ought not to rest on the unsupported testimony of any inventor. They ought, one would think, to be tested by independent men, refuted or confirmed; at all events, appraised at their proper value and significance.

I stand here to-night, not as the advocate of any particular system of ordnance, but in response to an expressed desire that you should be made acquainted with the physical experiments executed in connection with this subject. To others I leave the task of deciding on the national importance of these experiments. But I claim the right, if I deem it necessary to do so, of not only presenting the facts, but of expressing the thoughts which have occurred to me during the preparation of this discourse. From all that I have read and heard, I can come to no other conclusion than that for direct impact the ogival, or pointed projectile, possesses a higher penetrative power than the flat-headed one. Hence some of the statements made by the eminent mechanician who advocates the use of the latter must, I think, appear to a severe inquirer too unqualified in their assertion of its superiority. It is demonstrably superior at the oblique incidences above referred to; but from the comparison instituted by Sir Joseph Whitworth himself, and recorded in 'Guns and Steel,' pp. 46 and 47, it is evident that in the case of direct impact the ogival head exceeds the flat head in penetrative power. At an angle of 45° the latter clearly asserts its superiority. Hence, as the pointed head is best for penetration at an obliquity of 0°, and as the flat head is best at an angle of 45°, the thorough experimental examination would have to determine the obliquity at which both would be practically equal. The knowledge thus obtained, taken in connection with the exigencies of actual warfare, would be an important factor in the decision of the comparative merits of the two forms of projectile.

In the Preface to 'Guns and Steel' Sir Joseph Whitworth gives the following summary of his experiments on penetration:

"In 1857 I proved, for the first time, that a ship could be penetrated below the water-line by a *flat-headed* rifled projectile.

"In 1860 I penetrated, for the first time, a 4½-inch armour plate, with an 80 lbs. *flat-headed solid* steel projectile.

"In 1862 I penetrated, for the first time, a 4-inch armour plate, with a 70 lbs. *flat-headed steel shell*, which exploded in an oak box supporting the plate."

(I may remark that I was present when this experiment was made, and can testify that its result astonished those who witnessed it.)

"In 1870 I penetrated, with a 9-inch bore gun, three 5-inch armour plates, interlaminated with two 5-inch layers of iron concrete.

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* 'Guns and Steel,' p. 41.

"In 1872, with my new 9-pounder breech-loading gun, and a flat-headed steel projectile, I penetrated a 3-inch armour plate, at an angle of 45°.

"All these performances," continues Sir Joseph, "were the first of their kind, and were made, with one exception, with flat-headed projectiles." This exception, however, must be the important one, in which 15 inches of armour plating, and 10 inches of iron concrete, were penetrated by (I suppose) an ogival pointed shell. This, I take it, is the experiment described in pp. 51 and 52 of 'Guns and Steel.' It confirms the conclusion which I have been compelled to draw from the other experiments of Sir Joseph Whitworth above referred to. I could wish that this superiority of the ogival point, in the case of direct impact, had been more distinctly recognized; but I also wish to remind those whom I address that this is a point of detail, which in no way affects the merits of the Whitworth gun.

The tentative skill and insight which were so conspicuous in Mr. Whitworth's experiments with the rifle, are not less conspicuous in his experiments on rifled cannon. He takes the various elements of the gun in succession, and determines for each its condition of maximum efficiency. As before, he pushes his experiments through a range of variation so wide as to embrace both sides of this maximum. In 1856 he demonstrated that a rifle bullet should be 3 diameters long. "The rule," he affirms, "holds good for a 35-ton gun, as well as for a rifle." His experiments on the relation of the amount of twist to the length of the projectile are in the highest degree interesting. Projectiles varying from 1 to 7 diameters in length, yielded the following results:

"With one turn in 10 inches, all the projectiles went steadily with the point first.

"With one turn in 20 inches, the projectile became unsteady when more than 6 diameters in length.

"With one turn in 30 inches, it fell over when more than 5 diameters in length.

"With one turn in 45 inches, the projectiles turned over, and flew very wild, when more than 3 diameters in length."

The conclusion drawn from these experiments was, "that unless a gun be rifled with a quick pitch, so as to give a high rotation to the projectile, it would not be possible to fire long projectiles." With a sufficient twist, Mr. Whitworth succeeded, in 1856, in firing projectiles 10 diameters in length, and weighing 150 lbs.* For range, the best form of projectile has a conoidal front, a slightly tapered rear, and is from 3 to 4 diameters long. With high elevations, the flight of such a projectile may exceed, by a mile, that of one with its rear untapered. These two forms of the projectile are shown in Figs. 1 and 2.

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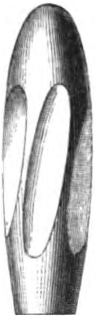
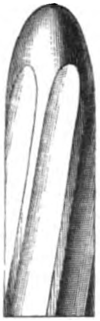
* A model of this bolt was present at the discourse. Instead of a long projectile a row of rifled spheres may be employed. They fly together, says Sir J. Whitworth, "like a covey of partridges, and would prove very formidable in attacking an enemy's boats."—'Guns and Steel,' p. 53.

FIG. 1.



TAPER REAR.

FIG. 2.



PARALLEL REAR.

The greatest range hitherto obtained was reached at Shoeburyness in 1868. It amounted to 11,243 yards, or nearly 6½ miles. The angle of elevation was 33°; weight of gun, 14 tons 8 cwt.; bore, hexagonal; major diam., 9 inches; minor diam., 8·2 inches; pitch, 1 turn in 165 inches;* weight of shot, 250 lbs.; length, 24·5 inches; powder charge, 50 lbs.

The possibility of attaining so great a range depends on the weight and cross-section of the shot. Were there no atmospheric resistance, the range would depend solely on the velocity of the projectile on quitting the gun. But if two projectiles, one possessing a greater transverse section than the other, start with the same velocity, the thicker bolt, encountering greater resistance, is brought more rapidly to rest. The velocity at starting may be even greatly in favour of the thicker projectile, while at long ranges it is left behind.

And here we are met by one of those practical reflections which force themselves upon the thoughtful mind, and in regard to which I have already claimed liberty of expression. If the object of the artillerist be solely to throw his shot with the maximum precision, to the greatest distance, then, so far as the data before me enable me to judge, Sir Joseph Whitworth has made out a conclusive case. In the common rifle, where each bullet is intended to kill or maim a single man, these two elements of range and precision are paramount. But in artillery practice another consideration comes into play. The use of shrapnel shell is one of the most important features of such practice. In this case vastness of range is not the only thing sought. At a certain point in its trajectory—a point which the practical artillerist might fix at two, three, or four thousand yards—the projectile has to burst, and spatter a rain of bullets round it. The effectiveness of such a projectile must obviously depend on the bursting charge which it is able to carry, and the number of bullets which it is able to scatter. If—and bear in mind that I use the "if"—if the projectile be so attenuated as to diminish seriously the bursting charge and the number of bullets, then it is easy to see that while it might assert a clear superiority as regards length of range, it might be less effective than a projectile of greater diameter, at the distances most advantageous for the use of shrapnel shell. I am far from saying that the Whitworth shell is unable to fulfil all the necessary conditions; but I do say that the shrapnel shell raises a question which was not raised in the case of the rifle. And were I on a committee entrusted with the decision of this question, I should require it to be proved that the very perfection of a gun, regarded from one point of view, is not an imperfection when regarded from another. The question is one for experiment to decide; and what those interested in the establishment of the truth will be most inclined to deprecate, is the closing of the door against experiment.*

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* This is not a quick pitch.—J. T.

Sir Joseph Whitworth has always advocated the use of steel in the construction of guns; but without some guarantee of its trustworthiness, he could hardly have expected to convert the world to his views. It would be unreasonable to expect military authorities to make their guns of a metal which, through some defect impossible to guard against, might at any moment convert the gun into a shell, scattering ruin among those who trusted it. The onus therefore rested upon the advocate of steel, to produce a metal which could be relied on. With the tenacity of purpose, and fruitfulness of inventive skill which characterized his whole previous career, Sir Joseph Whitworth attacked this problem. The solution of it will, perhaps, be best understood by giving you an account of an experiment which I witnessed at Manchester on last Saturday morning.

Within a hollow steel cylinder, of enormous strength, were placed a series of cast-iron bars, so as to form a kind of lining. The bars were laid loosely side by side, so as to admit of the passage of a gas between them. They were also grooved, with a view of facilitating gaseous motion. The bars were coated by a porous lining of sand and other materials, through which gases could readily be driven by pressure. In the middle of the cylinder stood a core, also formed so as to permit of the escape of gas from it. A space of several inches existed between the inner core and outward sheath. A large ladle was at hand, and into this was poured the molten metal from a number of crucibles. From the ladle again the metal was poured into the annular space just referred to, filling it to the brim. Down upon the molten mass descended the plunger of a hydraulic press. On first entering it a shower of the molten metal was scattered on all sides; but inasmuch as the distance between the annular plunger and the core on the one side, and the sheath on the other, was only about one-tenth of an inch, the fluid metal was immediately chilled and solidified. Thus entrapped it was subjected to pressure, which amounted eventually to about six tons per square inch.

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* Length of range, in the case of elongated projectiles, being obtained through the diminution of atmospheric resistance; the *fighting distance* at sea becomes a point of cardinal importance in reference to this question. If this distance be great, the elongated projectile triumphs; but if it be small, a short projectile with a high initial velocity, might be preferable.

Doubtless gases were here dissolved in the fluid mass, and doubtless also they were mechanically entangled in it as bubbles. I figure to myself the fluid metal as an assemblage of molecules, with the intermolecular spaces in communication with the air outside. Through these spaces I believe the carbonic oxide and the air to have been forced, finding their escape through the porous core on the one side, and through the porous sheath on the other.* From both core and sheath issued copious streams of gas, mainly, it would seem, in the condition of carbonic oxide flame. A considerable shortening of the fluid cylinder was the consequence of this expulsion of gases from its interior. The pressure was continued long after the gases had ceased to be ejected; for, otherwise, the contraction of the metal, on cooling, might subject it to injurious internal strains. In fact, castings have been known to be rent asunder by this contraction. By the continuance of the external pressure, every internal strain is at once responded to and satisfied, and the metal is kept compact.

The two main factors which determine the quality of any kind of steel are its strength and ductility. The method adopted by Sir Joseph Whitworth in determining these factors is, like all his mechanical contrivances, admirable. Both ends of a cylinder of a definite length and cross section are screwed firmly between two jaws, which are then separated by hydraulic pressure. For a time this stretched cylinder maintains a uniform diameter. At a certain pressure it passes its limit of elasticity, the passage being distinctly indicated by the dial which registers the pressure. From this point forward the cylinder is observed to contract at its centre, and it finally snaps across. The "strength" is measured by the breaking force; while the "ductility" is determined by bringing the fractured surfaces close together, measuring the length of the stretched mass, and expressing its elongation as a percentage of the original length of the cylinder. In one experiment made in my presence, forty seconds sufficed to stretch and break the cylinder, and there was not the slightest jar or jerk observed during the process. I entirely sympathize with the desire entertained by Sir Joseph Whitworth that these two elements of strength and ductility should be determined for, and registered upon, every ingot and bar of steel employed in the construction of our railway tires and axles; and, indeed, on all portions of machinery the giving way of which imperils human life.

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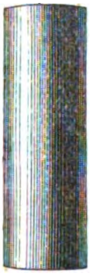
* The Florentine experiment, and the experiments of William Hopkins, show the power of pressure to force liquids through solids. The "core" is employed because of the difficulty of removing the air and gases from the heart of the ingot.

It may be added here that I have seen the fluid compressed steel in various stages of working, and sometimes in masses suitable for screw-propeller shafts, or for 35-ton guns. I have never detected a flaw or fault in it, its planed surfaces always disclosing a metal of the most closely coherent texture.

The metal employed for guns must be of the maximum "strength," compatible with a "ductility" which shall cause the gun to *tear*, if it should burst, and not fly into fragments. Sir Joseph Whitworth's method of testing the capacity of his metal to resist pressure is fully given in his work on 'Guns and Steel.' A powder charge is placed in a cylinder, both ends of which are stopped, the only exit for the gases being through the vent-hole. The annexed figures show the appearance presented by such a cylinder during the process of testing. 1, represents it before firing; 2, after firing a few rounds; 3, after bursting.* "The excellence of the metal was proved by the amount of powder used, and its tearing open in one piece."

1.



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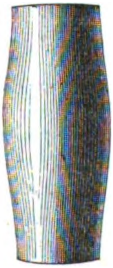

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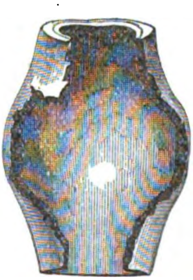

I wind up by a brief reference to Whitworth's breech-loading gun. It is thus described by its constructor: "This gun is made of fluid compressed steel, and the breech is closed by a heavy sliding block of the same material, working between two portions of the breech end of the gun, under and over the sliding block, which may be called block guides. The faces of the block guides are grooved with a number of parallel grooves, the upper and lower surfaces of the block being similarly formed, and inclined at a very small angle to a plane perpendicular to the axis of the gun, thus forming a portion of a screw thread which would be traced upon a cylinder of extremely large radius. These threads constitute what may be called a 'straight-line screw,' having their sides nearest the muzzle undercut, so that when the heavy strain resulting from firing the charge comes upon the block, there may be no tendency to force the block guides apart; but, on the contrary, they are more tightly held.

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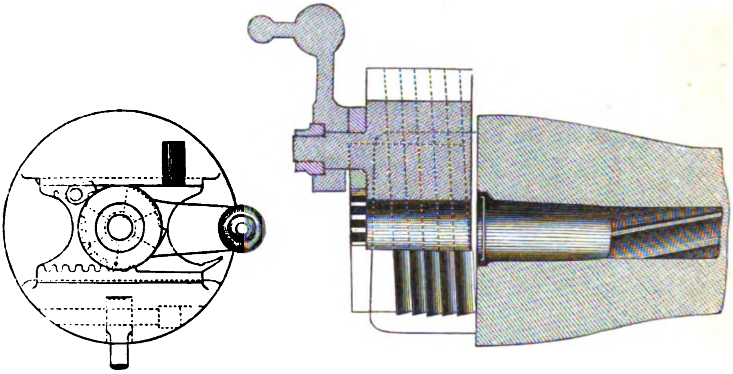
* Several examples of the bulged and torn cylinders were present in the discourse. The instruments used for determining whether any enlargement of the gun has taken place on firing were also present. An enlargement of $\frac{1}{10000}$th of an inch is thus discernible.



END VIEW, BREECH CLOSED. LONGITUDINAL SECTION, BREECH OPEN.

"The powder chamber is enlarged in diameter and reduced in length, being 2¼ diameters long, and containing a charge of 50 per cent. more powder than the service charge of a muzzle-loader. There is also a slightly enlarged shot-chamber to ensure ease in loading. The enlargement of the shot-chamber permits of the reduction of the windage in a degree which could not otherwise be attained. In this gun the enlargement is ·03 of an inch, the remaining part of the bore having windage of ·01 of an inch.

"The mechanical features of the breech-loader may be stated briefly as follows:

"1. A heavy breech-piece with a large area of bearing surface.
"2. An enlarged diameter of powder chamber.
"3. A slightly enlarged shot-chamber.
"4. A diminished windage in the bore of the gun."

Throughout this discourse I have left unnamed the celebrated man who, subsequently to the Crimean war, gave such an extraordinary impulse to the art and science of the artillerist. It would have been impossible in the limits here imposed on me to do the least justice to his achievements, and therefore I thought it better to leave them wholly untouched. This place I doubt not, and my services certainly, are open to him, should he desire to place an account of his labours before the members of the Royal Institution. It has been my privilege to enjoy the friendship of both the illustrious persons whose names have been so long before the public in connection with this question. I am conscious of having sought in an impartial spirit to give you the knowledge which you desired to obtain. In thus acting I have been of necessity restricted to the labours of one eminent inventor, who is here present. His great competitor is not present, and it is therefore all the more incumbent on me to remind you that, as far as I am concerned, his story of the guns remains untold; adding, if I might be permitted to do so, the expression of my admiration both of his character as a man, and of his genius as a scientific engineer.

[J. T.]

## GENERAL MONTHLY MEETING,

Monday, June 7, 1875.

WILLIAM POLE, Esq. M.A. F.R.S. Vice-President, in the Chair.

Thomas Cardwell, Esq.
Joseph Dalton Hooker, C.B. M.D. D.C.L. Pres. R.S. F.L.S. F.G.S. Correspond. Mem. Inst. France.
Francis Lloyd, Esq.
William Edwin Price, Esq. M.P.

were *elected* Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

*American Academy of Natural Sciences*—Proceedings for 1874. 8vo.
*Bombay Branch of the Royal Asiatic Society*—Journal, No. 30. 8vo. 1875.
*British Architects, Royal Institute of*—Sessional Papers, 1874–5. Nos. 10, 11. 4to.
*British Museum Trustees*—Catalogue of Marine Polyzoa. Part III. 8vo. 1875.
*Christiania Royal University*—Memoirs. 4to and 8vo. 1873–4.
  Postola Sögur (Sagas des Apôtres). Ed. C. R. Unger. 8vo. Christiania, 1874.
  J. Lieblein: Die Ægyptischen Denkmäler in St. Petersburg, Helsingfors, Upsala, und Copenhagen. 8vo. Christiania. 1873.
  Norsk Meteorologisk Aarbog for 1872 und 1873. fol. 1873–4.
*Coughtrie, Professor (the Author)*—Aerial Locomotion: Pettigrew *v.* Marey (Quart. Journ. Science, 1875).
*Cox, Mr. Serjeant, M.R.I. (the Author)*—The Province of Psychology. (K 101) 8vo. 1875.
*Editors*—American Journal of Science for May, 1875. 8vo.
  Athenæum for May, 1875. 4to.
  Chemical News for May, 1875. 4to.
  Engineer for May, 1875. fol.
  Journal for Applied Science for May, 1875. fol.
  Nature for May, 1875. 4to.
  Nautical Magazine for May, 1875. 8vo.
  Pharmaceutical Journal for May, 1875. 8vo.
  Practical Magazine for May, 1875. 8vo.
  Telegraph Journal for May, 1875. 8vo.
*Fife, M. S. Esq.*—S. Bent: The Thermal Gates to the Pole. 8vo. 1872.
*Franklin Institute*—Journal, No. 591. 8vo. 1875.
*Geographical Society, Royal*—Proceedings, Vol. XIX. Nos. 4, 5. 8vo. 1875.
  Journal, Vol. XLIV. 8vo. 1874.
*Geological Society*—Quarterly Journal, No. 122. 8vo. 1875.
*Linnean Society*—Journal, No. 59. 8vo. 1875.
*London Library Committee*—Catalogue of the London Library. 4th edition. 8vo. 1875.

*Mechanical Engineers' Institution*—Proceedings, Oct. 1874 and Jan. 1875. 8vo.

*Meteorological Society*—Quarterly Journal, New Series, No. 14. 8vo. 1875.

*Photographic Society*—Journal, Nos. 258, 259. 8vo. 1875.

*Preussische Akademie der Wissenschaften*—Monatsberichte: Register, 1859–73. Feb. 1875. 8vo.

*Royal Society of London*—Proceedings, No. 161. 8vo. 1875.

*Rossetti, F.* (*the Author*)—Confronto fra le Macchine Elettriche. (K 101) 8vo. 1875.

*Sandys, R. H. Esq. M.A.* (*the Author*)—"In the Beginning." Remarks on Certain Modern Views of the Creation. (K 101) 8vo. 1875.

*Sutherland, H. M.D. M.R.I.* (*the Author*)—On Sir H. Holland's Fragmentary Papers of Science (Journal Psychol. Med. N. S. 1). 8vo. 1875.

*Symons, G. J. Esq.* (*the Author*)—Symons' Monthly Meteorological Magazine, May, 1875. 8vo.

British Rainfall in 1874. 8vo. 1875.

*Tasmania Royal Society*—Monthly Notices, 1873. 8vo. 1874.

*Tyndall, Professor, D.C.L. F.R.S.* (*the Author*)—La Lumière: Traduit par Abbé Moigno. 8vo. Paris, 1875.

*United Service Institution, Royal*—Journal, Vol. XVIII. Appendix. 8vo. 1875.

*Vereins zur Beförderung des Gewerbfleisses in Preussen*—Verhandelungen. Mai–Dec. 1874. 4to.

*Wild, Dr. H.* (*the Director*)—Annales de l'Observatoire Physique Centrale de Russie, Année 1869 and 1873. 4to. 1874.

Repertorium für Meteorologie. Band IV. Heft 1. 4to. 1874.

*Wisconsin Academy*—Transactions, Vol. II. 8vo. 1874.

## GENERAL MONTHLY MEETING,

Monday, July 5, 1875.

THE DUKE OF NORTHUMBERLAND, D.C.L. President, in the Chair.

John Templeton Morgan, Esq.
John Bagot Scriven, Esq. B.A.
Sir Joseph Whitworth, Bart. LL.D. F.R.S.

were *elected* Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

*The Lords Commissioners of Admiralty*—Reports of the 'Challenger,' No. 3. fol. 1874.
*Actuaries, Institute of*—Journal, No. 98. 8vo. 1875.
*Astronomical Society, Royal*—Monthly Notices, Vol. XXXV. No. 7. 8vo. 1875.
*Bavarian Academy of Science*—Sitzungsberichte, 1875. Heft 1. 8vo.
*British Architects, Royal Institute of*—Sessional Papers, 1874–5, Nos. 12. 4to.
*Chemical Society*—Journal for May and June, 1875. 8vo.
*Editors*—American Journal of Science for June, 1875. 8vo.
  Athenæum for June, 1875. 4to.
  Chemical News for June, 1875. 4to.
  Engineer for June, 1875. fol.
  Journal for Applied Science for June, 1875. fol.
  Nature for June, 1875. 4to.
  Nautical Magazine for June, 1875. 8vo.
  Pharmaceutical Journal for June, 1875. 8vo.
  Practical Magazine for June, 1875. 8vo.
  Telegraph Journal for June, 1875. 8vo.
*Hayden, F. V. Esq. (the Author)*—Geological and Geographical Survey of Colorado. 8vo. Washington, 1874.
*Manchester Geological Society*—Transactions, Vol. XIII. Part 10. 8vo. 1875.
*Medical and Chirurgical Society, Royal*—Proceedings, Part 41. 8vo. 1875.
*Palestine Exploration Fund-Committee*—Our Work in Palestine since 1865. 12mo. 1875.
*Photographic Society*—Journal, No. 260. 8vo. 1875.
*Physical Society*—Proceedings, Part II. 8vo. 1875.
*Preussische Akademie der Wissenschaften*—Monatsberichte: März, 1875. 8vo.
*Roberts, W. C. Esq. F.R.S. M.R.I.*—Fifth Annual Report of the Deputy-Master of the Mint. 8vo. 1875.
*Royal Society of Literature*—Transactions. Second Series, Vol. XI. Part 1. 8vo.
*Symons, G. J. Esq. (the Author)*—Symons' Monthly Meteorological Magazine, June, 1875. 8vo.
*United Service Institution, Royal*—Journal, No. 80. 8vo. 1875.
*Victoria Institute*—Transactions, No. 33. 8vo. 1875.
*Warren, Major J. K. (the Author)*—Physical Features of the Minnesota River. (L 16) 8vo. 1874.
*Zoological Society*—Transactions, Vol. IX. Part 1–3. 4to. 1875.
  Proceedings, 1874, Part 4; 1875, Part 1. 8vo.

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